Government Banks and Interventions in Credit Markets*

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Abstract

We study a large-scale quasi-experiment in the Brazilian banking sector characterized by an unexpected and macroeconomically relevant increase in lending by commercial government owned banks. Using credit registry data, we find that this intervention led to a reduction in loan interest rates by private banks with limited effects in their credit supply. Firms reliant on government banks experienced a large increase in debt, and government banks faced a significant increase in default driven by levered firms. We find a small increase in employment at the firm level, suggesting limited direct benefits of the increase in credit by the government. At the regional level, we find that branch presence cannot explain credit growth due to cross-market borrowing. Once we account for this channel, we find real effects at the regional level that are larger than those at the firm level, but less than half of those that have been documented by the literature.

Keywords: Credit Market Interventions, Credit Supply Shocks, Government Banks

JEL Classification: E44, E65, G21, G28

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I. Introduction

Government interventions in credit markets, including interventions that make use of government banks, are ubiquitous.[1] The literature has identified benefits of interventions based on an increase in government lending, for example, to prevent a credit crunch during financial crisis or to counterbalance the role of information asymmetries. On the other hand, government interventions can lead to misallocation, be subject to political capture and sustain unproductive firms.[2] Nevertheless, there is no theoretical or empirical consensus on how increases in credit supply by government banks affect financial and real outcomes or on through which mechanisms these effects work, especially outside of crisis episodes.

We address these questions in a novel setting that exploits an unexpected and large-scale credit market intervention in Brazil. In March 2012, the Brazilian government announced, and shortly thereafter implemented, an increase in the credit supply of two of its largest commercial banks: Banco do Brasil (BB) and Caixa Economica Federal (CEF), which, together, were responsible for 38 percent of the outstanding credit in Brazil before the intervention. This intervention was targeted at Small and Medium Enterprises (SMEs), and it entailed an increase in credit at low interest rates by the government banks, which, it was presumed, would lead to a reduction in interest rates at private banks, and to an increase in credit access. The program was massive; the volume of outstanding credit from these two government banks increased about 30 percent in 2012, compared to a growth of 11.5 percent in outstanding credit from the largest private banks. We show that while there are benefits associated with this policy, such as a reduction in private banks' interest rates and modest positive employment effects, there were also substantial costs to the intervention, linked to an increase in corporate debt and higher loan default rates. To the best of our knowledge, ours is the first project to jointly and comprehensively study the response of loan quantities, prices and default, and the real effects of interventions that use government lending.

Our setting is ideal for studying a large-scale intervention in credit markets implemented

[1] Throughout this paper, we use the expressions *state-owned banks*, *government banks*, and *public banks* interchangeably. They refer to banks whose majority shareholder is a local or federal government and can lend directly to households and firms. We refer to banks that are not government banks as *private banks*.

[2] Papers highlighting a beneficial view of government interventions in financial markets include Stiglitz (1994), Tirole (2012), Philippon and Skreta (2012), Coleman and Feler (2015), and Eslava and Freixas (2021), among others. Papers that document a negative role for the state include Bertrand, Schoar and Thesmar (2007), Carvalho (2014), Acharya et al. (2019), Acharya et al. (2020), and Garber et al. (2022), among others.

through government banks. First, Brazil is a great laboratory in which to study credit market interventions. At that time in Brazil, bank lending represented nearly 52 percent of external finance, close to the current international average of 55 percent. Second, SMEs, which are the focus of the intervention and of our analysis, play a key economic role in terms of aggregate growth and employment. Throughout the world, these firms account for 60-70 percent of employment worldwide (Ayyagari, Beck and Demirguc-Kunt, 2007) and the majority of job creation in the United States (Neumark, Wall and Zhang, 2011). Third, we can isolate the specific mechanisms that affect credit allocation and firm outcomes, given that our experiment occurs outside of a crisis episode, and thus in a setting where firms and banks are not subject to any other systematic shocks. Finally, we have access to rich administrative credit registry data that are matched with employer-employee data, enabling us to provide comprehensive evidence of the financial and real consequences of the intervention.

The first part of the empirical analysis focuses on the effects of the intervention on lending rates and loan origination by government and private banks.[3] Public banks had lower loan interest rates than private banks both before and after the intervention, and the intervention was not characterized by large reductions in the interest rates of working capital loans made by government banks. Instead, the main mechanism of the intervention was a sudden increase in the supply of credit by government banks. Working capital originations grew by more than three times after the intervention was announced. Importantly, we find little evidence that political capture at the regional level was associated with the changes in the supply of public banks' credit.

Second, we turn to the response of private banks, relying on the fact that the intervention was unexpected and that no other systematic events that could affect the behavior of private banks took place at the time. Private banks responded to the increase in the supply of credit by public banks by reducing their lending rates. As a consequence, the difference between the lending rates of public and private banks fell about three percentage points, corresponding to 20 percent of the pre-intervention difference between private and public banks' lending rates. Although we do find a reduction in the lending rate, we do not find an aggregate increase in the credit supply from private banks during the same period. In fact, we find that

[3]Throughout the paper, we focus mainly on working capital loans, which are term loans that can be used for any purpose and represent one of the main sources of external funding for firms in the Brazilian economy.

firms that borrowed exclusively from public banks experienced a substantially larger growth of their outstanding debt after the intervention relative to firms that borrowed exclusively from private banks. While this increase in debt-to-payroll is relatively smaller for firms that borrowed from both private and public banks, we document a reduction in their debt obtained from private banks, suggesting crowding out of private credit.

We then analyze the delinquency rates on government and private banks' loan portfolio, revealing several noteworthy patterns. First, the intervention was associated with an increase in the delinquency of loans originated by government banks relative to those originated by private banks. Prior to the intervention, government banks had delinquency rates marginally smaller than private banks. However, after the intervention, the probability that a loan from a public bank would become delinquent was, on average, 100 basis points higher than the average delinquency probability for private bank loans. This translates into a 20 percent higher probability of delinquency for firms borrowing from public banks relative to firms borrowing from private banks. Given the differences in the interest rates of public and private bank loans, if markets were characterized by severe adverse selection public banks would have attracted safer borrowers both before *and* after the intervention. Our findings instead suggest adverse selection is not the main driver of riskiness in our context. Moreover, we find that this increase in public banks' delinquency was entirely driven by levered firms. This finding suggests that government banks did not relax their credit standards as part of the intervention, since new borrowers from both public and private banks have similar delinquency, but rather that the increase in corporate leverage caused by the intervention led to a subsequent increase borrower delinquency for public banks.

Next, we explore the real effects associated with the intervention at the firm-level. The challenge is that borrowers from public banks faced an increased credit availability, while borrowers from private banks faced a decrease in the cost of their new loans. In other words, both types of borrowers were, to some extent, treated by the policy. To deal with this problem, we exploit the structure of relationships in the data and perform two comparisons. First, we compare borrowers that had exclusive relationships with public banks with those that had exclusive relationship to private banks throughout the sample. Second, we compare borrowers that had relationships with both types of banks with those that had exclusive relationship

to private banks throughout the sample.[4] We find an increase in about 1 percentage point in employment growth in the subset of non-exclusive borrowers, but no effect when comparing exclusive public and private bank borrowers. Since non-exclusive borrowers benefit from *both* the reduction in interest rates of private banks *and* an increase in credit availability by public banks, our estimates of the difference in employment growth for non-exclusive borrowers allows us to isolate the direct effects of the credit supply shock by public banks. Nevertheless, the employment effects we document at the firm level are roughly one third of the estimates of these effects in the credit supply literature and small given the size of the intervention.

To avoid potential selection effects at the firm level and to better account for general equilibrium effects, we also explore the effects of the policy at the regional level. We first focus on a set of municipalities that allow for the cleanest identification of the intervention and its effects. This sample consists of local banking markets that had no new entry of previously absent banks and that are either public or private monopolies (that is, have branches from only one bank). Our identifying assumption is that branch presence in the baseline and exposure to the intervention was independent of changes in credit demand following the intervention. Since bank entry into most of these municipalities was the result of bank privatizations that took place several years before our analysis, it is unlikely they were correlated with changes in economic conditions. Following the intervention, we find that credit outstanding in branches in those municipalities grew substantially, growing approximately 20 percentage points more in public relative to private monopolies, consistent with aggregate date. We do not find, however, that the volume of credit outstanding for firms located in public monopolies grew more than those in private monopolies, which can result from firms located in private monopolies borrowing from branches in public monopolies. Not surprisingly, we do not find any effect on real outcomes of having a public bank branch at the moment of the intervention.

We address this cross-market borrowing channel by using an alternative measure of exposure to the intervention. This measure is the share of the outstanding volume of working

[4] In both cases we condition our analysis in the subset of firms that do borrow after the intervention. While there are potential selection issues associated with demand for credit and employment growth, our identification hypothesis is that these selection problems are the same across firms, regardless of whether their pre-policy exposure was to public or private banks.

capital loans from public banks in a given municipality at the baseline. Using this alternative measure, we find that regions where all of the outstanding working capital loans were from public banks before the intervention experienced a 63 percent higher credit growth and a 4.65 percent higher GDP growth, relative to regions where none of the outstanding working capital loans were from public banks before the intervention. Importantly, this is an implied elasticity of output to credit supply of 0.07. We also find significant effects in terms of employment and payroll, emphasizing the general-equilibrium effects of large-scale interventions. Although these effects at the regional level are larger than those at the firm level, they are smaller than those estimated in the credit supply literature. Herreño (2021), for instance, finds a 0.2 elasticity of aggregate output to a credit supply shock. These limited positive effects combined with increased delinquency and firm leverage cast doubt on the effectiveness of interventions outside crisis episodes.

Related Literature This study adds to the broad literature that studies government interventions in financial markets.[5] Closer to our paper, Jiménez et al. (2019) analyze a small credit facility of a Spanish state-owned bank during the 2008–2009 financial crisis. Jiménez et al. (2019) find that although this facility attracted riskier borrowers, the social value of such intervention during the crisis was still positive. Our project differs from their empirical evidence in two major ways. First, the intervention we analyze is not a response to a crisis episode, and there were no other large shocks affecting the decisions of banks and borrowers. Second, the episode we analyze was macroeconomically large, allowing us to study broader implications for equilibrium in credit markets, including the response of private banks, the endogenous changes in the pool of borrowers, and aggregate real effects of the intervention. This provides us with a more comprehensive perspective on the benefits *and* costs of increases in the supply of credit by the government. We show that large-scale government-induced credit expansions outside of economic downturns have limited direct and indirect real effects, and we cannot rule out the idea that the intervention led to credit misallocation.

Our paper also contributes to the literature on credit supply shocks. The empirical litera-

[5] See, for instance, Gale (1990), Bertrand, Schoar and Thesmar (2007), Veronesi and Zingales (2010), Acharya et al. (2020), and Acharya et al. (2021). For interventions implemented during COVID-19 pandemic, including interventions using state-owned banks, see Granja, Leuz and Rajan (2022), Altavilla et al. (2021), Jimenez et al. (2022) and Huneeus et al. (2022).

ture has primarily focused on negative credit supply shocks (for example, Khwaja and Mian (2008), Chodorow-Reich (2014), and many others), especially following the Great Financial Crisis. Understanding the role of positive credit supply shocks is important, as there are theoretical reasons why their effects would be different from those of credit crunches, and booms are the best predictors of financial crisis (for example, Freixas, Laeven and Peydró (2015)). One notable exception is Jiménez et al. (2020), who study a credit boom in Spain and find no quantity increase in credit supply at the firm level and, as a consequence, no real effects as well. Contrary to Jiménez et al. (2020), we provide novel evidence of the strength of the real effects of credit booms in a quasi-experimental setting where credit, in fact, grows significantly at the firm level. Another relevant study is Fonseca and Van Doornik (2022), which studies a bankruptcy reform in Brazil that led to increased credit availability for firms. Our analysis complements their findings by providing direct evidence of the effects of a credit supply shock in the form of a large and unexpected increase in government lending, which allows us to study the effects of positive credit supply shocks to competitors' interest rates and loan amounts, and to study the dynamics of default rates.

Our paper is also related to the literature studying government owned banks, some of which also analyze the Brazilian banking sector. Several papers document some form of political capture of government banks and consequent misallocation (La Porta, Lopez-De-Silanes and Shleifer (2002), Sapienza (2004), Dinç (2005), Carvalho (2014)).[6] In contrast with these projects, we find little evidence of political capture at the regional level as a driving force determining the allocation of loans by government banks.[7] Fonseca and Matray (2022) uses an expansion in branch coverage by government owned banks to study the effects of financial deepening on economic development and wage inequality. In contrast, the policy we study is characterized by a sudden and systematic increase in credit availability by government banks, and provide us with an opportunity to explore the competitive effects and unintended consequences of large expansions in government lending. Closer to our project, Garber et al. (2022) show that the same credit expansion that we study led to an increase in

[6] See also Assuncao, Mityakov and Townsend (2012), Bertay, Demirgüç-Kunt and Huizinga (2015), Ru (2018), Bircan and Saka (2021) and Cao et al. (2022). Other projects focusing on Brazil include Coelho, De Mello and Rezende (2013), Lazzarini et al. (2015), Sanches, Silva Junior and Srisuma (2018) and Schmitz (2020).

[7] See Appendix B for more details on our analysis of potential regional political capture. Additionally, several papers study the use of state-owned banks during the Great Recession to prevent a credit crunch, including Coleman and Feler (2015), Cortes, Silva and Van Doornik (2019) and Capeleti, Garcia and Sanches (2022)

household debt that ultimately led to smaller consumption during the 2014–2016 economic downturn. Our paper complements the evidence in Garber et al. (2022) by studying the response of private banks, connecting the increase in credit supply to changes in firm leverage and delinquency, and exploring the real effects of the policy at the firm and regional levels.

Finally, our paper also contributes to the regional banking literature. The evidence on credit supply shocks at levels of aggregation above the firm is still mixed (e.g., Mian, Sufi and Verner (2019) and Nguyen (2019) in the U.S. and Huber (2018) in Germany). As argued in Ashcraft (2005), Huber (2018) and others, one reason for this finding is that there is no heterogeneity in regional exposure to large, systemic shocks. Our evidence suggests this is indeed the case in our setting when exposure is measured by physical branch presence, but that once we also account for firms' locations we can estimate the effects of this particular large-scale intervention. More broadly, our evidence suggests that even if small business lending is mostly local (for instance, see Granja, Leuz and Rajan (2022)), when analyzing large shocks it is fundamental to account for the location of both the bank and the borrower, as well as the potential for the expansion (or contraction) of credit markets as a function of the shock itself.

II. Data Sources

Our main source of data is the credit registry data from the Brazilian central bank matched with employment and payroll data from the Annual Review of Social Information (RAIS). We complement these matched data with publicly available data from various sources as outlined below.

Credit registry data are from the Credit Information System (SCR) of the Central Bank of Brazil. Banks are required to disclose to the Brazilian central bank loan-level data for all outstanding loans with amounts above a specific threshold (at the time of origination),[8] allowing us to observe the near universe of loans to firms in Brazil. The database includes detailed information about loan contracts, such as the type of credit, interest rate, amount, maturity, and collateral, as well as some basic information at the firm level (such as firms' time-invariant taxpayer identifiers). We restrict the analysis to loans funded by banks' own

[8] R$ 5,000 (around $2,500) until December 2011; BRL 1,000 (around $500.00) from January 2012 onward.

resources.[9,10] The data also allow us to track delinquency and firms' credit history. Since loan identifiers are not constant across time, we track delinquency information at the firm-month of origination-loan type-bank dimension, up to one year after origination.[11] Following Jiménez et al. (2014), we mark a loan as delinquent if it is more than 90 days past due.

The firm-level employment and payroll data that we use in our analysis are from the Annual Review of Social Information (RAIS). All tax-registered firms in Brazil are required to complete a form in which they provide individual labor contract information for each of their employees. Given the severe penalties firms face for incomplete or late filings of the form, the RAIS covers the universe of all tax-registered firms. We aggregate these data at the firm-employee level to obtain employment and total payroll at the firm level. We merge the SCR and RAIS data based on the firm's time-invariant taxpayer identifier. Each dataset contain firms without a correspondent in the other dataset. Not all firms have access to credit and/or decide to borrow in a given year (are in RAIS but not SCR), and non-employer firms that do borrow are in SCR but not in RAIS. The latter corresponds to less than 15 percent of the total amount originated by government banks as part of the intervention, and are not included in our sample. We use employment headcount to construct firm-size categories.[12]

Throughout the paper, we also use other publicly available data with information on banks' balance sheets, branch locations, and regional variables at the municipality level. Banks' balance sheets, income statements, and regulatory capital information for all financial institutions in the country are available at a quarterly frequency at the Central Bank of Brazil's IF.data website. Branch balance sheet data containing detailed assets and liabili-

[9] We construct a time series of loan originations by looking at all loans originated in a given month that have a positive amount outstanding at the end of that month. While we exclude very short-term loans in the process, the majority of the corporate loans have maturities of more than one month.

[10] We can separate loans funded by banks' internal resources (deposits and capital) and external resources. This distinction is important since development banks in Brazil fund a significant amount of loans using commercial banks as intermediaries (see, for instance, Lazzarini et al. (2015)).

[11] Our approach for measuring delinquency is comprehensive, despite the constraints, for two reasons. First, most of the firms in our sample are small and thus have a unique month of origination-loan type-bank loan in the sample. Second, our definition of borrower quality reflects lenders' information for a given firm for a given type of credit, which is the economically relevant dimension.

[12] We follow the classification by Brazilian Support Service for Small and Medium Enterprises (SEBRAE). Micro firms: firms in the service/commerce sectors with fewer than 10 employees, or in the industry sectors with fewer than 20 employees. Small firms: firms in the service/commerce sectors with more than 10 and fewer than 50 employees, or in the industry sectors with more than 20 and fewer than 100 employees. Medium firms: firms in the service/commerce sectors with more than 50 and fewer than 100 employees, or in the industry sectors with more than 100 and fewer than 500 employees.

ties information at the branch level are available at a monthly frequency from the Monthly Bank Statistics by Municipality (ESTBAN). ESTBAN data also include the municipality of each branch and thus allow us to identify the entry and exit of banks in each municipality. Finally, we use population and output data from the Brazilian Institute of Geography and Statistics (IBGE).

There are five types of corporate loans that most commercial banks provide using their own funding sources: working capital, discounted receivables (loans where firms anticipate the receipt of cash flows from sales and other accounts receivables), auto loans, credit cards, and overdraft accounts. Our paper focuses primarily on working capital loans for three reasons. First, they were, together with discounted receivables, the focus of the intervention. Second, working capital loans are the primary source of funds for firms, accounting for roughly 50 percent of the loan volume in our sample before the intervention (March 2012) and 60 percent by the end of 2013. Third, they have longer maturities than discounted receivables, which allows us to track borrower delinquency over time more accurately. Within the set of working capital loans, we focus primarily on the uncollateralized ones.[13] These are the majority of working capital loans in our sample.

Table 1 shows borrower summary statistics from our data. Panel A illustrates how large the differences are between the interest rates charged by private banks and those charged by public banks. Interest rates for working capital loans issued by public banks are more than 10 percentage points lower than interest rates charged by private banks. Loans issued by public banks are also smaller in size and have longer maturities. Panel B provides a breakdown of firm characteristics based on their relationships over the whole period. Firms that borrow from private banks only are larger than firms borrowing exclusively from public banks, on average, and have more debt outstanding. Firms with access to both types of banks are larger, consistent with the notion that such firms benefit less from exclusive relationships with banks.

[13] Strictly speaking, some of the loans in our sample are guaranteed by the owners of the firm, as individuals. As this type of guarantee is extremely weak, we consider them as uncollateralized.

III. Institutional Setting and Intervention Details

After the Great Financial Crisis in 2008-2009 Brazil experienced a fast economic recovery, with the country's GDP growing 7.5 percent in 2010 and 4.3 percent in 2011 (Figure A.1, Panel (a)). President Dilma Rousseff became president in January of 2011 and increased the number of policies geared towards avoiding an economic slowdown. Monetary policy became expansionary, with the policy rate going from 12.50 percent in July 2011 to 7.25 percent in October 2012 (Figure A.1, Panel (b)). As evidenced by the summary statistics of our data (Table 1), lending rates in Brazil were high in our sample period. These rates (and the implied spread over the deposit rate) were high even when compared with those of other developing countries. The lending spread in 2011 was 32.9 percentage points in Brazil, compared with 3.4 percentage points in Argentina and 3.7 percentage points in Mexico, for example.[14] Even after the reduction in policy rates after July 2011 and other regulatory changes, rates for consumers and small businesses stayed at high levels, which led the government to further intervene in the Brazilian banking sector.

In March 2012, the government announced that it would use two state-owned banks—Banco do Brasil (BB) and Caixa Economica Federal (CEF)—to promote credit supply increases for several types of loans, both to consumers and firms, at lower interest rates.[15] These actions were taken through various separate government programs. Two prominent examples are "Bom pra Todos," which was implemented by BB, and "Caixa Melhor Credito," implemented by CEF. As these programs were large and broadly unexpected, in the two weeks following their announcement, BB stock prices fell 7.62 percent. Initially, both BB and CEF had balance sheet capacity to increase their credit supply and originate these loans at lower lending rates, as the reduction in margins was being compensated for by increases in volume. By the end of 2012 public banks' credit outstanding had grown approximately 30 percent, compared with an increase of 11.5 percent in outstanding credit from private banks.

The reasoning behind the intervention was that by increasing the amount of credit provided by government banks and charging lower interest rates, the government would suc-

[14] Source: IMF International Financial Statistics.

[15] State-owned banks were, and still are, large players in the Brazilian financial sector. Although there are differences in how these banks are managed and in their ownership structure (for instance, BB has publicly traded shares while CEF does not), both institutions are controlled by the Brazilian government and can be actively used as a means to implement credit policies.

cessfully increase the competitive pressure on private banks. Achieving lower interest rates was a fundamental goal for Brazil's economic policymakers, who held the belief that lower interest rates were necessary for sustainable economic growth and would prevent a slowdown of economic activity.[16] Importantly, we do not find that the intervention was motivated by political concerns (see, for instance, the net approval rating on Figure A.1, Panel (c)), high stock prices or large exchange movements (Figure A.1, Panels (e) and (d)), inflation expectations (Figure A.1, Panel (f)), or other macroeconomic factors. In Appendix B we discuss the heterogeneity in credit growth across municipalities. We show that funds were not disproportionately allocated to municipalities with mayors from the party of the president even with a mayoral election taking place in October 2012. We do find a higher than expected allocation in municipalities where the previous election was close, but this effect is smaller relative to the overall credit growth in the intervention and present for less than 2 percent of the municipalities in our sample. These results reduce the concerns that the allocation of public loans was systematically driven by political capture. Moreover, we show that credit growth differences between municipalities with higher industrial or agricultural shares, more or less private banks credit concentration, or to municipalities with higher GDP or credit per capita was also small relative to the overall increase in credit from the intervention.

By mid-2013, however, macroeconomic conditions changed, and banks expected a significant tightening of financial conditions on the horizon. Government officials indicated that the public banks were no longer able to keep the same pace of credit increases due to lack of balance sheet capacity and risk of default.[17] Although the intervention does not have an official end date, we focus our analysis on the period from 2011 to 2013. By the end of 2013, public and private banks started increasing lending rates and public banks had significantly reduced the pace of their increase in credit supply (Figure A.4).

The effects of the intervention, and its undoing, can be directly observed in banks' balance

[16] Former President Roussef was particularly unhappy with the high interest rates. For instance, in her 2012 Labor Day speech, which took place shortly after the intervention started, she said, "The Brazilian economy will only be completely competitive when our interest rates (...) match the interest rates employed in international markets (...) It is unacceptable that Brazil, which has one of the most stable and profitable financial sectors in the world, continues to have one of the highest interest rates (...) government banks proved that it is possible to reduce interest rates in loan operations, credit cards and even payroll loans. It is important that Private Banks follow suit." Source: https://g1.globo.com/economia/noticia/2012/04/dilma-critica-altas-taxas-de-juros-e-diz-que-bancos-tem-logica-perversa.html

[17] For instance, see https://www.valor.com.br/financas/3017518/governo-ve-limite-para-bb-e-caixa and https://www.valor.com.br/financas/3023666/bancos-federais-chegam-ao-limite-da-baixa-de-juro.

sheet data. In Figure 1, Panel (a), we show the change in the volume of outstanding credit (relative to March 2012) issued by the two public banks in the program (BB and CEF) and the five largest private banks in Brazil.[18] We observe a significant credit expansion from public banks. We do not find a similar increase in credit from private banks. In Figure 1, Panel (b), we show the change in the other assets in banks' balance sheets. We do not observe that the intervention-driven credit increase was associated with a contraction of other parts of public banks' balance sheets, or a differential trajectory of other assets between public and private banks.

We find that this increase in credit was funded by various sources. In Figure A.5, we show that there was a larger increase in deposits and no differential increase in equity at public banks relative to private banks, suggesting that the intervention was partly funded from deposits. However, we show in Figure A.6 that the share of deposits relative to total liabilities fell for both types of banks, and the asset increase was mainly funded through a mix of onlending (mostly of government funds) and security issuance. Although BB and CEF are controlled by the government, both banks were profitable (from an accounting perspective) before the intervention, and their return on assets was in line with those of banks in comparable economies.[19] In Figure A.7, we see that a few quarters after the intervention there was a large increase in public banks' ROA due to their increased lending activity. This trend was reversed when the economy slows down and delinquency increases.

The effects of the intervention can also be observed in new originations in the credit registry data. In Figure 2, Panel (a), we see a large jump in the originations of working capital loans right after the beginning of the intervention. Despite a sudden and large increase in government banks' lending, we do not observe an immediate large reduction in the volume of loans originated by private banks. Figure 2, Panel (b) shows the average interest rates of working capital loans of public and private banks. The notion that government banks were able to provide loans at lower interest rates is evident in Figure 2, Panel (b). We can see that government banks provided substantially cheaper credit relative to private banks, both

[18] We use only the five largest private banks so that we have a comparable group of financial institutions. Together, these seven financial institutions are responsible for more than 80 percent of the volume of credit outstanding in the baseline in our data.

[19] For instance, the average ROA of banks in Chile was around 1.5 percent in 2012, while the joint ROA of BB and CEF was 1.2 percent. The average across all OECD countries during the period was around 0.5 percent. Source: IMF's Global Financial Development database.

before and after the intervention. Moreover, right after the beginning of the intervention we observe a large decrease in the interest rates of private banks. Despite the decline, the difference in interest rates between private and government banks remained large after the intervention, with private loans being, on average, 12.8 percentage points more expensive before the intervention and 7.4 percentage points more expensive after the intervention.[20]

As a consequence of the increase in new originations, we also observe in the credit registry data an increase in the outstanding amount of working capital loans from public banks (Figure 3, Panel (a)). Consistent with our narrative description of the intervention, the outstanding amount of working capital loans grows at a fast pace until the end of 2012 and then slows in 2013. In Figure 3, Panel (b) we show that there was also an increase in the amount outstanding of discounted receivables, but the scale of the increase is 2.5 percent of the increase in the amount outstanding of working capital loan. The aggregate evidence suggests private banks responded by reducing interest rates but not increasing (and, if anything, decreasing) their credit supply. In the next section, we confirm these results, exploiting the richness of the credit registry data.

IV. Lending Rates, Firm Debt, and Delinquency

In this section, we estimate on the effects of the intervention at the loan and firm levels. We focus on the effects on lending rates, firm debt, and delinquency.

IV.1. Interest Rates and Debt

At the core of the intervention was the government belief that private banks would respond to the additional competition by their public counterparts by reducing interest rates on their loans. The aggregate evidence in Figure 2 indicates that there was a reduction, but it was not enough to bring the difference between public and private banks' interest rates to zero. These aggregate differences can reflect borrower or loan characteristics among which private and government banks differ or change in response to the intervention. To account for this possibility, we focus on individual loan issuance, and compare loans issued by private and

[20] We also find evidence that public banks price discriminate based on firms size less that private banks do, as can be seen in Figure A.9 and Table A.4. This dispersion in financing spreads can also have implications for financial development (see Cavalcanti et al. (2021)).

government banks before and after the intervention, while controlling for firm and contract specific features and a broad range of fixed effects.

Our setup resembles a difference-in-differences specification, but one in which both types of banks were affected by the intervention. Although this can pose an additional hurdle for identification of the effects of the intervention, the context of our analysis allows us to confidently state that there are no other systematic shocks that could cause meaningful changes in the difference between private and public interest rates. In particular, there were no large mergers, bank failures, or other macroprudential policies that would affect different banks differently. Furthermore, the absence of a financial crisis means we do not have to worry about the different behavior of private and government banks—or their borrowers—during such episodes. Therefore, our identification assumption is that, given the absence of any systematic shocks that hit private and government banks differently, changes in the difference between private and government banks' interest rates were caused by the intervention.[21] Formally, we estimate Equation (1) at the loan level:

$$i_{jtmbfs} = \alpha_{tms} + \alpha_{bf} + \alpha_{t,j(maturity)} + \alpha_{t,f(size)} + \sum_{\tau \neq 0} \delta_\tau \, Private_b + \varepsilon_{jtmbfs} \tag{1}$$

where i_{jtmbfs} denotes the interest rate of a loan j issued in month t in municipality m by bank b to firm f in industry s. $Private_b$ is a dummy equal to one if bank b is a private bank, α_{tms} are time-municipality-industry fixed effects, α_b are bank fixed effects, $\alpha_{t,j(maturity)}$ are time-maturity fixed effects, and $\alpha_{t,f(size)}$ are time-firm-size fixed effects. For loan maturity and firm size, we bin the underlying continuous variables in several different categories. [22] We weight the regressions by loan volume. The coefficients of interest are δ_τ, the differential change in interest rates charged by private banks relative to public banks. The use of a broad range of fixed effects guarantees that we are comparing loans in the same region and month

[21] One related concern is that foreign banks are high by shocks that are unobserved to us, possibly related to sovereign debt crisis in Europe. While there is evidence that foreign banks to such episodes reducing their cross border lending and lending by subsidiaries (De Haas and Van Horen (2013) and Schnabl (2012)), this is less worrisome our context since foreign banks hold a small market share (close to 10%) of working capital loans.

[22] We classify firms' industry according to their 2-digit *CNAE* classification. For details, see IBGE website. For firm size, we use the micro, small, medium and large definitions from Section II. For loan maturity, the bins are: one to three months, three to six months, six to nine months, nine to twelve months, and then for maturities of more than a year we create six month bins until forty-two months and one final bin for working capital loans with maturities longer than forty-two months.

and for firms in the same industry that have the same size, and that firm-bank specific characteristics are also accounted for. Additionally, time-maturity fixed effects guarantee that we are comparing loans with the same maturity issued in the same month. In summary, our specification ensures our analysis is not capturing changes in the composition in banks' loan portfolios in response to the intervention. We also estimate a version of Equation (1) with firm-time fixed effects, as in Khwaja and Mian (2008). This alternative specification accounts for firms credit demand by comparing the lending rate for the same firm by different bank types and delivers similar results. Importantly, this specification severely limits the sample in our analysis since it requires that the SMEs in our sample originate working capital loans from more than one type of bank in a given month.

The results in shown in Figure 4. The results in Figure 4, Panel (a) indicate that private banks' lending rates fell sharply relative to those of government banks after the intervention. The spread between private and public banks' interest rates fell about 2.7 percentage points on average in the post intervention period.[23] This is a reduction of about 20 percent of the pre-intervention difference between private and public interest rates. We find a quantitatively similar result if we estimate Equation (1) with firm-time fixed effects (Figure 4, Panel (b)), indicating that our benchmark set of fixed effects can account for credit demand.

Next, we focus on the effects of the intervention on firm debt. We use all types of debt outstanding to capture potential substitution between working capital loans and other types of credit. Since we do not have balance sheet information, we use payroll as a measure of firm size. Specifically, we define debt-to-payroll as a firm's outstanding debt divided by its payroll costs in 2011. We then estimate a difference-in-differences specification to understand how the debt-to-payroll ratio of borrowers from public banks changes relative to that of borrowers from private banks, as in Equation (2):[24]

$$\frac{\text{Debt}_{tf}}{\text{Payroll}_{2011,f}} = \alpha_t + \alpha_f + \sum_{\tau \neq 0} \gamma_\tau \cdot Public_f + \varepsilon_{tf}, \tag{2}$$

[23] We also find suggestive evidence that the interest rate reduction caused by the intervention is larger for micro firms relative to small, medium and large firms, as seen in Table A.4).

[24] We opt to estimate this regression relative to a firm-size measure rather than simply in the logs for two reasons. First, we are not as interested in the growth of credit as we are in the size of this growth relative to the firms' operations. Second, since there is a large increase in credit in a previously small segment from public banks, the log-growth nonlinearity can bias our results. A similar scaling of debt at the individual level in the context of consumer debt is used by Garber et al. (2022).

where the dependent variable is the oustanding debt of firm f in month t relative to its total payroll in 2011, α_t and α_f are time and firm fixed effects, $Public_f$ is a indicator that is one if firm f is a borrower from a public bank and γ_τ are the coefficients of interest. We estimate Equation (2) for two different samples. First, we consider firms with exclusive relationships with private and public banks throughout the sample. Second, we compare firms with non-exclusive relationships with those that have exclusive relationships with private banks. For the latter sample of firms, we also estimate Equation (2) with debt originated by private banks relative to payroll as a dependent variable. This allows us to test if borrowers with non-exclusive relationships reduce their reliance on private debt after the intervention, relative to borrowers with exclusive private relationships. For both samples, we restrict our analysis to a balanced panel of firms to avoid picking up changes in the composition of the pool of borrowers.

The results are shown in Figure 5. The increase in funding availability caused by the intervention has a remarkable effect on the debt of firms with exclusive relationship with public banks (Panel a). The debt-to-payroll ratio of firms that borrowed from government banks increases substantially relative to firms who only borrowed from private banks. The coefficient estimate of 1.14 in December 2013 indicates that firms borrowing from public banks experienced an increase on average of 1.14 times their annual payroll relative to those that borrowed exclusively from private banks. For reference, firms that borrow exclusively from public banks had a baseline average level of debt-to-payroll ratio of 4.14 in March 2012, such that the increase corresponds to roughly 27 percent of their baseline level.

Figure 5, Panel (b), shows a qualitatively similar, but quantitatively smaller effect when comparing firms that borrow from both banks with firms that borrowed exclusively from private banks. The coefficient estimate of 0.72 in December 2013 implies a 12 percent increase in their level of debt-to-payroll relative to their baseline level of 5.75 in March 2012. This smaller effect is explained by the reduction in debt obtained from private banks, as can be seen in Figure 5, Panel (c). In particular, firms that borrow from both types of banks reduce their volume of debt outstanding from private banks by 0.63 of their annual payroll relative to firms borrowing from private banks exclusively, which suggests within-firm crowding out of private debt. Non exclusive borrowers had a private debt to payroll ratio of 3.58 on average in March 2012. Thus, our estimates translate to a reduction of 17 percent

in non-exclusive firms private debt-to-payroll relative to exclusive private borrowers. Since interest rates for these loans from private banks came down after the intervention, one could actually expect private debt to also increase. However, the intervention can be seen as a relaxation of a constraint in the supply of credit provided by the government, which in level are still cheaper than loans offered by private banks. Facing an increase in the availability of cheaper funds, we find that firms with non-exclusive relationships would choose to increase their share of loans from public banks.

Although throughout this section we keep the denominator fixed as firms' payroll in 2011, one important questions is whether firms that were borrowing more were also hiring more workers and increasing their payroll. We come back to this issue when we analyze the real effects of the intervention. Moreover, the substantial increase in debt-to-payroll we document can have an effect on firms' ability to comply with their financial contracts, which can lead to higher delinquency rates for government banks, an issue we turn to next.

IV.2. Borrower Risk and Delinquency

To understand how the intervention affected delinquency rates we to compare the its trajectory for public and private banks before and following the intervention. We say a firm that borrowed in a given month from a certain bank was delinquent if any of the loans from that bank to that firm in that month became delinquent for more than 90 days within a year after origination.[25] For example, if a firm obtained a loan in May 2012, we track that firm's delinquency until May 2013. If the firm failed to pay its loan installments for at least 90 days on this one year window, we define the firm as delinquent on loans it contracted in May 2012.

We first analyze the average delinquency over time for private and government banks separately. Formally, we estimate the following specification at the firm-bank level:

$$D_{tmbfs} = \alpha_{ms} + \alpha_b + \alpha_{f(size)} + \sum_{\tau \neq -1} \gamma_\tau + \varepsilon_{tmbfs}, \tag{3}$$

where D_{tmbfs} is an indicator equal to one if a loan originated in month t in municipality m from bank b to firm f in industry s becomes delinquent within one year after origination,

[25] The choice of a 90-day cutoff follows other papers in the literature, such as Jiménez et al. (2014) and Jiménez et al. (2019).

α_{ms} are municipality-industry fixed effects, α_b are bank fixed effects, $\alpha_{f(size)}$ are firm-size fixed effects, and γ_τ are time dummies. Each γ_τ indicates the average change in delinquency probability at a given month relative to March 2012.

The results are shown in Figure 6. Prior to the intervention, public and private banks have very similar delinquency rates relative to their respective baseline levels, despite large differences in the average interest rate of their borrowers. However, after the intervention, government banks experienced a deterioration of their loan portfolio, while the delinquency rate on private banks' loans initially improves and eventually goes back to its pre-intervention level. From a quantitative perspective, the effects we find are large. Before the intervention, 5 percent of borrowers were delinquent. Although we do not find any increase in delinquency for loans originated right after March 2012, his difference increases for loans originated later on. For loans originated after October 2012 we find a two percentage point increase in the likelihood of default. For loans originated in December 2013, we find an increase of over three percentage points. If we aggregate all of these coefficients weighting by the volume of loans originated in each period we arrive at a one percentage point increase, on average, in the likelihood of default in loans extended by public banks relative to private banks by the end of 2013. We confirm these findings in a difference-in-differences specification where we estimate Equation (3) for both types of banks jointly with time-municipality-industry fixed effects. The use of time-municipality-industry fixed effects guarantees that the differences in delinquency are not explained by shocks that affect firms in the same month that are in the same location and operating in similar industries (for instance, credit demand shocks).[26] The results of this difference-in-differences specification are shown in Figure A.10.

To understand the source of the relative increase in delinquency of loans originated by public banks, we redo our analysis of public and private banks' delinquency over time for levered and unlevered firms separately. A levered firm is a firm that has any outstanding debt when the new working capital loan is originated. Figure 6, Panel (b) shows that levered borrowers from government banks became delinquent more often than firms that borrowed from private banks during the intervention. By contrast, Panel (c) shows that new borrowers of public and private banks had comparable risk, both before and during the intervention.

[26] One implication of the use of these controls is that our results are not explained, for example, by the fact that government banks entered many new locations during the intervention.

This indicates that the increase in default for government banks can be explained by the increase in default from previously levered firms. By facilitating credit access to already levered firms, government banks exposed themselves to greater risk of borrower delinquency. This view incorporates the notion that more levered firms are more likely to become delinquent, which is true in our sample (Figure A.11).

Our results cast doubt on the idea that an asymmetric information mechanism that generates a negative relationship between interest rates and borrower quality is at play. Despite lower interest rates, public banks attracted borrowers whose risk was similar (higher) to that of borrowers from private banks before (after) the intervention.[27] Given the differences between the interest rates of public and private banks, one would expect that government banks would have attracted borrowers that were safer than those that private banks attracted, both before and after the intervention, which is not what we observe in the data.

Furthermore, our results allow us to can rule out the hypothesis that government banks relaxed their credit standards for all borrowers as part of the intervention, since new borrowers from both public and private banks have similar risk.[28] This is in contrast to the evidence in Jiménez et al. (2019), for example, that document that government banks accepted applications from borrowers with worse ex ante credit scores in a similar intervention implemented in Spain during the financial crisis. Our results are consistent with the idea that by increasing credit supply during the intervention, public banks favored levered firms that eventually became delinquent, leading to a deterioration in public banks' loan portfolio. Loans to unlevered (mostly new) borrowers accounted for 14 percent of public banks' working capital originations before the intervention and only 7 percent afterward (Figure A.12). We do not find a similar change in working capital loans made by private banks.

[27] Another possibility is that lower interest rates attracted riskier borrowers, such as in the advantageous selection models (De Meza and Webb (1987), Mahoney and Weyl (2017), Biswas and Koufopoulos (2019)). Similarly, lower interest rates can have a causal effect on borrower risk, as in moral hazard models, which would imply a lower risk level for government-bank borrowers (Boyd and De Nicoló (2005), Martinez-Miera and Repullo (2010)).

[28] We also perform one additional test to rule out the possibility that adverse selection is driving our results. We compare firms that had or had had a relationship with a bank in the two years preceding the intervention with fist-time borrowers and find that, conditional on having no outstanding debt, firms with previous relationships and new borrowers had similar risk, contradicting the hypothesis that new borrowers, which would be attracted by lower interest rates, are safer (riskier), as in adverse (advantageous) selection models.

V. Real Effects of the Intervention

In this section we study the effects of the intervention on real outcomes, both at the firm and regional levels. A priori, firm and regional level analysis do not have to deliver the same results, and are both important economically. The distinction comes from the fact our regional analysis encompasses local general equilibrium effects, credit reallocation across firms and changes in borrower composition, all of which are controlled away in our firm level analysis. At the firm level, we estimate the employment effects based on firms' exclusive and non-exclusive relationships with public and private banks. At the regional level, we focus on two questions. First, we study to what extent credit grew more in municipalities more exposed to the intervention. Second, we study whether there were real effects on local output, employment and payroll as a consequence of the regional change in credit conditions and how do these effects compare to those at the firm level.

V.1. Firm Level Employment Effects

The magnitude of the policy and the corresponding changes in equilibrium pose a challenge to the estimation of real effects at the firm level since both public *and* private borrowers benefit from the intervention - the former from accessing more credit, the latter from access to cheaper credit. We address this challenge by exploring how firms with exclusive and non-exclusive relationships behave. First, we compare public and private borrowers that had exclusive relationships with either type of bank throughout the sample. Firms with exclusive relationships with public banks benefited from increased credit availability (Figure 5, Panel a). On the other hand, firms with exclusive relationships with private banks faced a stronger decline interest rates following the intervention (Figure 4, Panel a). Second, we compare firms that had non-exclusive relationships with public banks throughout the sample with those that had exclusive relationships with private banks. This set of firms also benefited from the increase in credit availability (Figure 5, Panel b) *and* were also was exposed to interest rate decreases following the intervention (Figure 4, Panel b). The first test allows us to understand if either effect (increase in credit supply or the reduction in interest rates) dominates each other. The second comparison allows us to isolate the effect of the increase in public credit on real outcomes, since non-exclusive borrowers are subject to both effects.

In both comparisons we constrain our analysis to firms that do borrow at some point in 2012 or 2013, to allow for at least one year of post treatment and to capture actual exposure to the policy. A natural concern is that, by constraining our analysis to the subset of firms that *do* borrow, we introduce bias in our estimates since job growth and demand for credit can correlate for reasons other than the intervention. The important difference is that, in our case, *both* treatment and control firms would be subject to the same unobserved bias related to demand for credit. Thus, the identification hypothesis is that firms that borrow from private and public banks do not differ in how their demand for credit is correlated with job growth. For each comparison, we estimate the following regression at the firm-year level:

$$y_{tmsf} = \alpha_{tmsf(size)} + \alpha_f + \beta_{2012} Public_f \cdot I_{2012} + \beta_{2013} Public_f \cdot I_{2013} + \epsilon_{tmsf} \tag{4}$$

where the dependent variables are log-growth of employment and log-growth of payroll in year t of a firm f that operates in sector s and is located in city m, $\alpha_{tmsf(size)}$ are time-municipality-industry-firm size fixed effects, α_f are firm fixed effects, $Public_f$ is an indicator equal to one if the firm had a borrowed exclusively from public banks (Panel A), from both types of banks (Panel B), or if this firm did not have any prior outstanding debt but borrowed from a public bank following the intervention (Panel C), and I_{2012} and I_{2013} are indicator variables for years 2012 and 2013. The coefficients of interest, β_{2012} and β_{2013}, capture the relative change in employment and payroll growth in years 2012 and 2013 relative to employment and payroll growth in 2011.

The results are shown in Table 2. For firms with exclusive relationships with either public or private banks (Panel A), there is no change in neither employment and payroll growth, suggesting the credit growth effect and the interest rate effect have similar magnitudes. In Table 2, Panel B, we perform the same exercise, but comparing firms that borrowed from both types of banks with firms that borrowed exclusively from private banks. While coefficients for 2012 are modestly negative and statistically insignificant, coefficients for 2013 suggest a 1.1 percentage point larger increase in employment growth for non exclusive borrowers when compared to exclusive borrowers. The effect was stronger for small firms relative to micro firms. This result might result from the fact that smaller firms were more productive at the margin. Importantly, one can interpret these estimates as a direct effect of the additional

credit provided by government banks, as both non-exclusive borrowers and firms that only borrow from private banks benefit from lower interest rates from these banks.

The employment effects we document in this section are modest relative to other estimates in the credit supply literature focusing on credit crunches. For instance, Huber (2018), Chodorow-Reich (2014), and Bentolila, Jansen and Jiménez (2018) find that firms that depended on distressed banks reduced employment growth by four to six percent. Huber (2018), for instance, finds this decrease in employment for a 16 to 20 percent decrease in credit. Studying a credit boom in Spain, Jiménez et al. (2020) finds no increase in credit supply at the firm level and, as a consequence, no real effects as well. Nevertheless, to address challenges associated with selection of firms into different banks and potential general equilibrium effects, we extend our analysis using credit and real effects at the regional level.

V.2. Regional Level

We now explore the effects of the intervention at the regional level. We consider a municipality in Brazil to be our benchmark definition of a local banking market.[29] The identify the effects of the intervention on credit growth and real outcomes, we use municipalities heterogenous exposure to public banks before the intervention. Our first measure of heterogeneous exposure is an indicator variable based on the presence of branches from public banks in a given municipality. Our identifying assumption is that branch presence in the baseline is independent of changes in credit demand following the intervention.

Our benchmark samples includes municipalities where there were only branches from one bank (either private or public) and no branch openings of previously absent banks in that municipality in our sample period. These municipalities provide the cleanest experiment to measure the effects of the intervention on credit and real outcomes at the regional level. Bank entry into most of these municipalities was the result of M&As and privatization processes at the state and national levels that took place several years before the beginning of our sample. In other words, it is unlikely that branch location is correlated with changes in current economic conditions. We also find that local monopolies from private banks are comparable to those from public in various dimensions, such as agricultural and industry

[29]This is the same definition as in Sanches, Silva Junior and Srisuma (2018) and Coelho, De Mello and Rezende (2013) and various other papers that study the Brazilian banking sector.

shares, and GDP per capita, reinforcing our identification assumption. This sample includes 894 of the 3398 municipalities with at least one bank branch in the baseline.[30]

To measure the credit effects of the intervention at the regional level, we estimate the following regression at the municipality m, quarter t level

$$y_{mt} = \alpha_m + \gamma_{t,s} + \sum_{\tau \neq -1} \beta_\tau Public_m + \varepsilon_{mt} \tag{5}$$

where y_{tmr} is a measure of credit (or credit growth) originated in municipality m, quarter t, α_m are municipality fixed effects, $\gamma_{t,s}$ are state-time fixed effects and $Public_m$ is the municipalities' exposure to the intervention. We estimate Equation (5) with various measures of credit growth. In particular, we use as dependent variables the logarithm of the outstanding volume of working capital loans to firms in a given municipality using credit registry data, regardless of the branch location, and the volume of credit outstanding in branches in a given municipality using Bank Branch data, regardless of the firm location. Standard errors are clustered at the state-level.

Figure 7 shows the estimates of differences in within-municipality credit growth from Equation (5). We find a large relative increase in the outstanding amount of credit at branches (Panel a) in municipalities that had branches from public banks. The magnitude of the change in total credit outstanding from these branches is consistent with the overall change in credit following the intervention. However, we do not observe a similar growth in the outstanding volume of working capital loans for firms in that municipality (Panel b). This apparent puzzle can be explained by cross-municipality borrowing. To see that, we estimate the following regression at the municipality m, quarter t level:

$$\Delta \ln (\text{WK Outstanding})_{b,m,t} = \alpha + \sum_{\tau \neq -1} \delta_t + \varepsilon_{b,m,t} \tag{6}$$

where $\ln (\text{WK Outstanding})_{b,m,t}$ is the log of the outstanding volume of working capital loans for firms in municipality m from bank type b (public or private) at quarter t. We estimate Equation (6) separately for the municipalities with branches from private and public banks separately and display the results in Figure 8. We see that the outstanding volume of work-

[30] Results are also shown for a more representative sample of municipalities later on.

ing capital loans from public banks grew significantly in municipalities with or without branches from public banks, while the outstanding volume of working capital loans from private banks grew only in municipalities with branches from private banks. In other words, firms in municipalities with only private banks increased significantly their borrowing from both public and private banks, while firms in municipalities with only public banks did not increase their borrowing from private banks. The end result is that the volume outstanding of working capital loans grew more in locations without a public bank branch.

To account for this cross-municipality borrowing in our analysis, we construct a measure of exposure to the intervention based on the baseline share of the outstanding volume of working capital loans that were originated by public banks to firms located in a given municipality. While our previous exposure measure is based on the locations of banks' branches, this second measure uses the locations of the banks' borrowers. Importantly, the share of the outstanding volume of working capital loans from public banks is relatively stable before the intervention, such that it is unlikely that it is correlated with changes in economic conditions. The within-municipality standard deviation in this share is 0.08 relative to an average of 0.63. We replicate the results of Figure 7 and re-estimate Equation (5) with this alternative measure of exposure $Public_m$. The results are shown in Figure 9. We find that credit grew significantly more in municipalities with a higher share of the outstanding volume of working capital loans from public banks at the baseline. Consistent with the cross-municipality borrowing channel, we find a smaller increase in credit outstanding in branches in municipalities using this alternative measure of exposure to the policy (Panel b).

To match our analysis of real outcomes, we also estimate Equation (5) at the annual level using data from December of each year, that is

$$y_{mt} = \alpha_m + \alpha_{t,s} + \beta_{2012} Public_m \times I_{2012} + \beta_{2013} Public_m \times I_{2013} + \varepsilon_{mt} \tag{7}$$

where I_{2012} and I_{2013} are year indicator variables, and the rest of the terms are the same as in Equation (5). The results are shown in Table 3. We confirm our previous findings that used quarterly data. We find that municipalities with branches from public banks experienced a larger growth in credit outstanding at those branches, but in fact a relative reduction in the outstanding volume of working capital loans made to firms in those municipalities. Once we

use this alternative exposure measure, we find that credit outstanding for firms in municipalities with a higher exposure grew significantly and much more so than credit originated in branches at the same municipalities. Quantitatively, we find that the outstanding volume of working capital loans grew 63 percent more in municipalities where all of the outstanding amount of working capital loans were from public banks before the intervention relative to those where none were. Alternatively, we find that a one standard deviation change in the exposure to the intervention (standard deviation of 0.3) was associated with a 16 percent higher growth in credit outstanding for firms in that municipality.

To understand the real effects of the intervention, we estimate Equation (7) with the log of GDP, employment, and total payroll as the dependent variables. The results are shown in Columns 1-3 of Table 4. We find no differential real effects in municipalities with branches from private banks (Panel A). This is not surprising given that we do not find an increase in credit to firms in those municipalities. We find a GDP growth that was 4.65 percent higher in municipalities where all of the working capital loans were from public banks before the intervention relative to those where none were. Alternatively, we find that a one standard deviation change in the share of the outstanding amount of working capital loans from public banks before the intervention was associated with a 1.4 percent higher GDP growth. We find quantitatively similar (although not statistically significant) effects on employment and payroll in these municipalities.

We find similar results in terms of credit growth and real outcomes in a broader sample of municipalities. For this alternative sample, we consider municipalities where there was no branch openings of previously absent banks during our sample period. Contrary to our benchmark sample, municipalities in these sample had any combination of public and private banks' branches. Although in this sample the effects of the intervention are potentially not as well identified as in the case of local monopolies, this sample is much more representative and includes 2785 municipalities. We find the same results in terms of credit growth (Table A.1) and real outcomes (Columns 4-6 of Table 4). We also replicate our analysis using pre-intervention population as weights in the credit growth and real outcomes regressions and find similar results (Tables A.2 and A.3). Overall, we find larger real effects at the regional level relative to our firm level evidence. This is qualitatively consistent with the result in Huber (2018) that the direct effect of a credit crunch episode accounted for 25 percent of

the regional effects. The fact that our firm level evidence points to similar effects on employment and payroll and our regional level evidence points to smaller effects on employment relative to payroll indicate that the intervention had a large effect on local wages through local general-equilibrium effects.

Our results also have far reaching implications for the literature that uses regional exposure based on branch location in the banking literature. Although there is evidence that credit markets for small businesses are typically local (see, for example, Granja, Leuz and Rajan (2022) and Li and Strahan (2021)), we show that when analyzing macroeconomically large shocks, one must account for the location of both the bank and the location of the borrower, as well as the potential for the expansion of credit markets as a function of the shock itself.[31] As argued in Ashcraft (2005), Huber (2018) and others, a reason why some papers do not find large effects of credit supply shocks at the level of aggregation beyond the firm is that there is no heterogeneity in regional exposure to large, systemic shocks. Our evidence suggests that this problem of heterogeneity in exposure is particularly acute if exposure is measured by physical branch presence, but that once we account for firms' locations the real effects of positive credit supply shocks can be identified and, in our setting, are significant at the regional level.

VI. Conclusion

In this paper we study a credit market intervention implemented by the Brazilian government using public banks. The intervention is characterized by a large and unexpected increase in the supply of credit to firms at lower interest rates, and implemented during a period where the economy is growing and neither banks nor borrowers are in distress. The combination of this unique quasi-experiment and our the availability of detailed data allow us to jointly analyze the implications of the intervention for lending rates, loan originations, debt outstanding, default and real effects at firm and regional levels.

We document that the intervention was associated with a large increase in loan originations and debt from public banks and a reduction in lending rates from private banks. Firms

[31] In the Brazilian context, researchers should be cautious when using the location of the branch from ESTBAN instead of the location of the borrower from SCR. This mismatch tends to increase over time with the fast pace of banking services digitalization.

that obtained loans issued by government banks during the intervention were more likely to default on those loans than comparable firms that borrowed from private banks. This deterioration in the quality of government banks' loan portfolios is connected to loans issued to levered firms, which were favored in the allocation of loans in the program. We rule out alternative explanations as to why the intervention is characterized by the worsening in credit quality, such as selection or poor screening by government banks. We find that despite a large relative increase in credit, the intervention had only a small real effect at the firm level.

At the regional level, we find that branch presence cannot account for the increase in credit for firms in a given municipality. We provide evidence of cross-municipality borrowing in response to the intervention. We show that once we account for borrowers' location, we observe large increases in credit at the regional level based on pre-intervention exposure to the intervention. We find real effects at the regional level that are substantially larger than the within-region firm level effects, pointing to significant general equilibrium and spillover effects. However, these regional effects are still smaller than those in the credit supply literature, pointing to the the low effectiveness of government interventions in credit markets outside of crisis episodes. Beyond the estimated effectiveness of the intervention, our results suggest that the empirical banking literature that estimates the effects of large shocks must account for both borrower and branch location and the potential or large cross-market borrowing in general and as a response to the shocks themselves.

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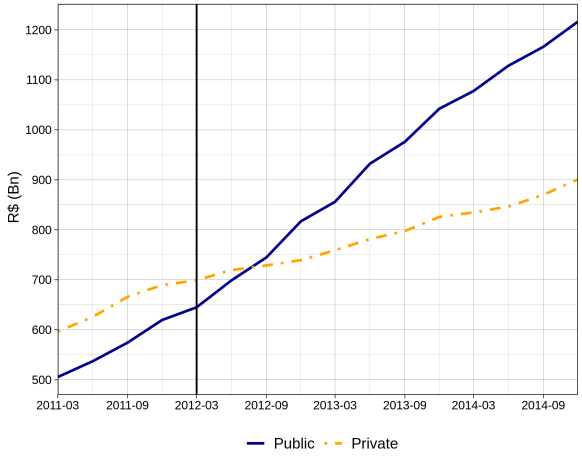
Table 1: Summary Statistics

This table reports summary statistics for the main variables in our data set. There are N_{obs} =2.6M observations and N_{firms} = 793,121 firms in the matched sample. Our sample period is from 2011 to 2013. Sources: Credit Information System (SCR), RAIS, and authors' calculations.

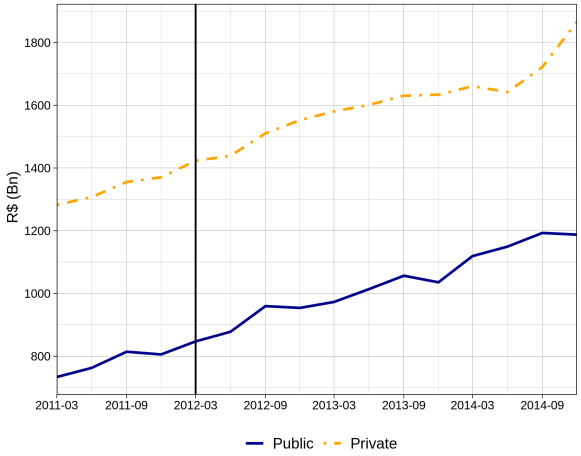
Variable	Mean	Median	SD
Panel A: Loans			
Panel A.1 - Public Banks Loans			
Amount (R$)	62422	36268	92095
Maturity (months)	23.17	24	10
Interest Rate (APR)	25	23.63	10.69
Panel A.2 - Private Banks Loans			
Amount (R$)	84868	33847	143916
Maturity (months)	16.09	15	10.60
Interest Rate (APR)	37.75	34.48	16.62
Panel B: Firms			
Panel B.1 - Firms that borrow exclusively from Public Banks			
Num. of Employees	10.17	4	40.13
Payroll Costs (R$ per Month)	11,666	3,738	57,023
Total Outstanding Debt	97,833	27,218	1,041,000
Debt-to-Payroll Ratio	2.56	0.61	12.8
Panel B.2 - Firms that borrow exclusively from Private Banks			
Num. of Employees	11.42	3.01	66,37
Payroll Costs (R$ per Month)	14,659	3,074	98,539
Total Outstanding Debt	168,655	11,398	2,132,000
Debt-to-Payroll Ratio	2.27	0.33	16.5
Panel B.3 - Firms that borrow from both types of Banks			
Num. of Employees	18.66	6.5	76.9
Payroll Costs (R$ per Month)	23,084	6,545	97,106
Total Outstanding Debt	392,461	111,134	1,829,000
Debt-to-Payroll Ratio	4.48	1.63	16.7

Figure 1: Outstanding Credit and Other Assets: Public and Large Private Banks

This figure shows the volume of outstanding credit and other assets by bank type by quarter. *Public* (government-owned) banks are *Banco do Brasil* (BB) and *Caixa Economica Federal* (CEF). Private banks are: Bradesco, HSBC, Itau Unibanco, and Santander. Total volume outstanding includes all outstanding credit to firms and households. The vertical line indicates the start of the intervention (2012Q1). Sources: IF.data and authors' calculations.



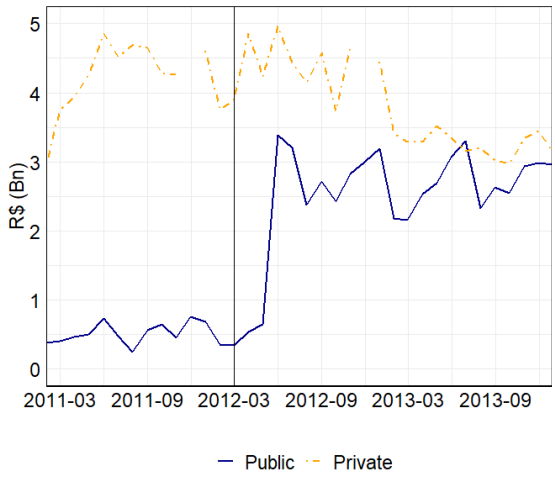
(a) Outstanding Credit



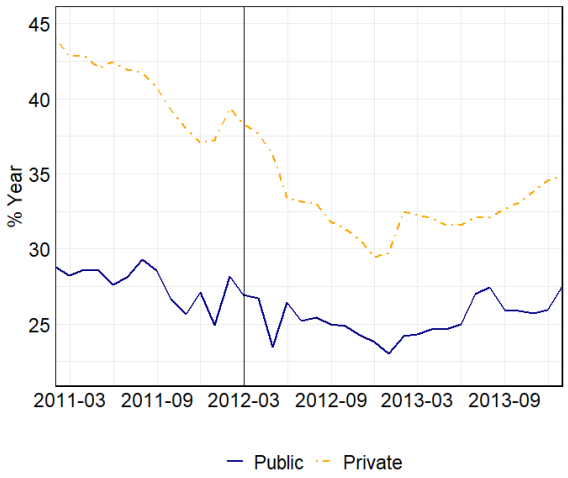
(b) Other Assets

Figure 2: Working Capital Origination and Interest Rates: Public and Private Banks

This figure shows the volume and interest rates of monthly origination of uncollateralized working capital loans to firms by type of bank. *Public* (government-owned) banks are *Banco do Brasil* (BB) and *Caixa Economica Federal* (CEF). Private banks are all other banks that are not controlled by the government. Interest rate is shown as Annual Percentage Rate (APR). The vertical line indicates the start of the intervention (March 2012). Sources: Credit Information System (SCR), and authors' calculations.



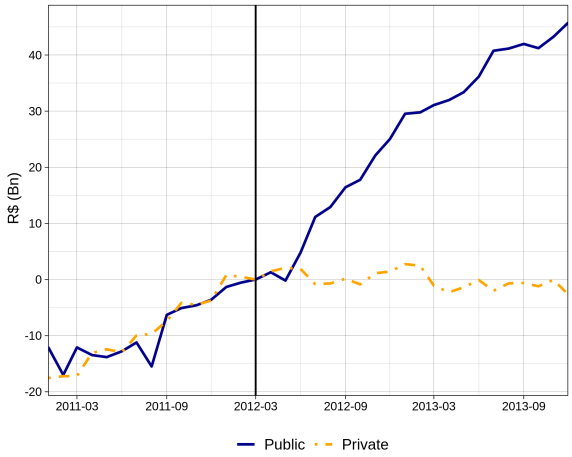
(a) Volume



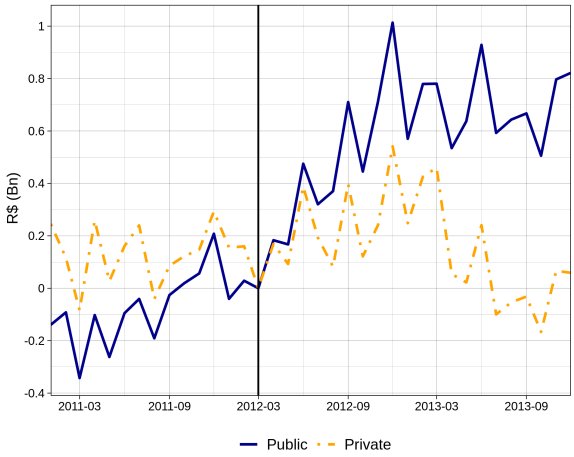
(b) Interest Rates

Figure 3: Change in Total Credit Outstanding: Working Capital and Discounted Receivables

This figure shows the quarterly volume of loans outstanding to firms by type of bank. *Public* (government-owned) banks are *Banco do Brasil* (BB) and *Caixa Economica Federal* (CEF). Private banks are all other banks that are not controlled by the government. Panel (a) shows the change in the amount of uncollateralized working capital loans and Panel (b) shows the change in the amount of discounted receivables relative to baseline (March 2012). The vertical line indicates the start of the intervention. Sources: Credit Information System (SCR), and authors' calculations.



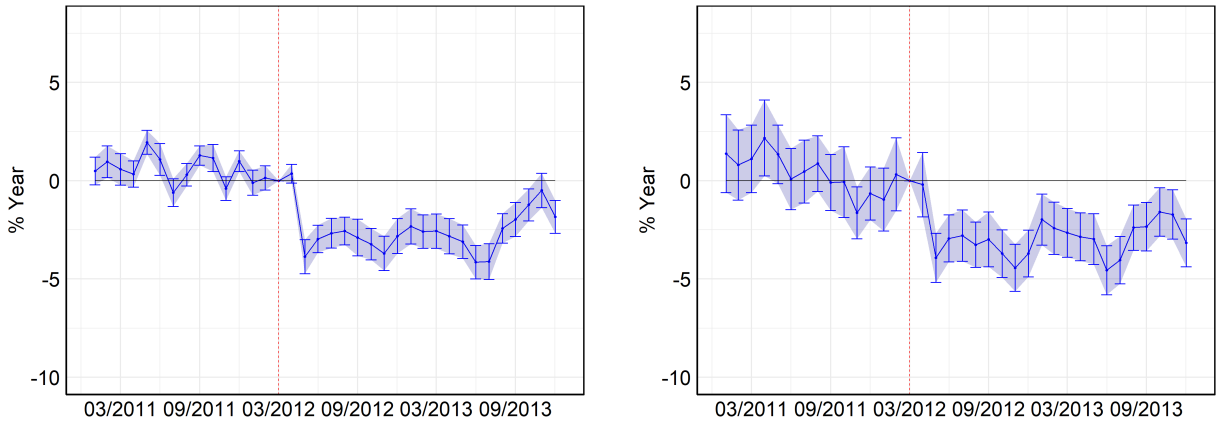
(a) Working Capital



(b) Discounted Receivables

Figure 4: Differential Interest Rate Changes: Public and Private Banks

This figure shows the estimates of δ_τ from Equation (1) at the loan level, with March 2012 as the reference month (vertical line), weighted by loan amount. For **Panel (a)**, we run: $i_{jtmbfs} = \alpha_{tms} + \alpha_{fb} + \alpha_{t,j(maturity)} + \alpha_{t,f(size)} + \sum_{\tau \neq -1} \delta_\tau \, Private_b + \varepsilon_{jtmbfs}$, where i_{jtmbfs} denotes the interest rate of a loan j issued in month t municipality m by bank b to firm f in industry s, α_{tms} are time-municipality-industry fixed effects, α_b are bank fixed effects, $\alpha_{t,j(maturity)}$ are time-maturity fixed effects, $\alpha_{t,f(size)}$ are time-firm size fixed effects, and $Private_b$ is an indicator equal to one if bank b is a private bank. For **Panel (b)**, we replace the firm-bank fixed effects by bank and firm-time fixed effects Standard errors are clustered at bank-municipality level. Shaded areas are the 95 percent confidence intervals. Sources: Credit Information System (SCR), and authors' calculations.

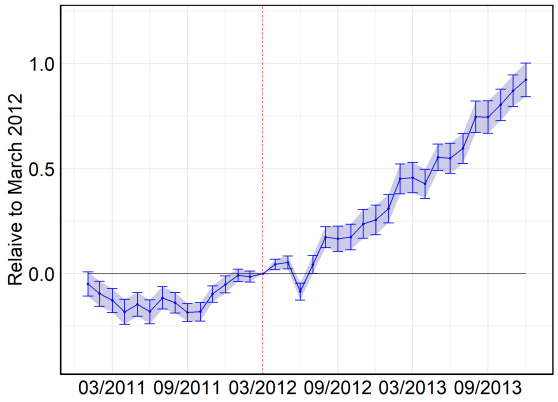


(a) Firm-Bank Fixed Effects

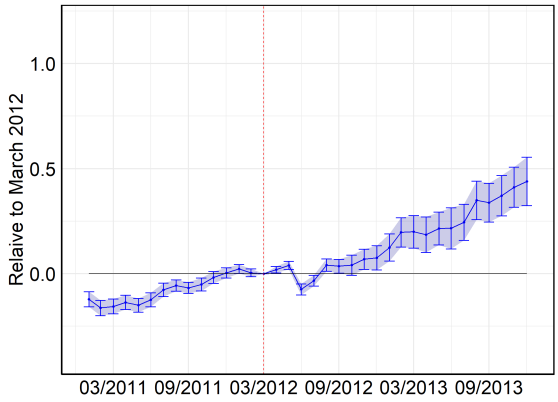
(b) Firm-Time Fixed Effects

Figure 5: Debt-to-Payroll Ratio: Difference-in-Differences Specification

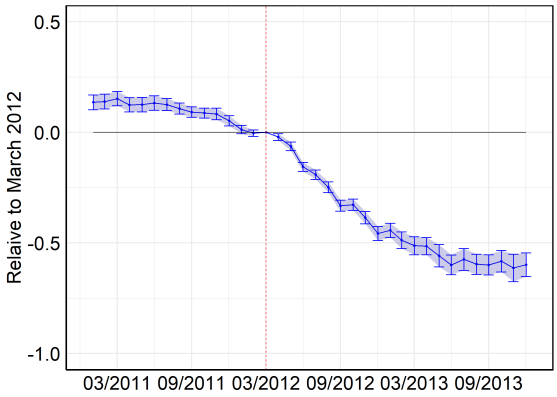
This figure shows the estimates of δ_τ from Equation (2) at the firm level, with March 2012 as the reference month (vertical line). **Panel A**: the sample consists of firms with exclusive relationships with types of banks. **Panels B and C**: the sample consists of firms that borrow from both types of banks with firms that have exclusive relationships with private banks. More specifically, for each sample, we run: $Debt\ to\ Payroll_{tf} = \alpha_t + \alpha_f + \sum_{\tau \neq 0} \gamma_\tau \cdot Public_f + \varepsilon_{tf}$, where $Debt\ to\ Payroll_{tf}$ denotes the debt-to-payroll ratio of a firm f in month t, α_t, and α_f are time and firm fixed effects, and $Public_f$ is an indicator if the firm has a relationship with a public bank. **Panels A and B**: the dependent variable is total outstanding debt. **Panel C:** the dependent variable is debt outstanding from private banks only. Standard errors are clustered at the bank-municipality level. Shaded areas are the 95 percent confidence intervals. Sources: Credit Information System (SCR), Annual Review of Social Information (RAIS), and authors' calculations.



(a) Exclusive Relationships



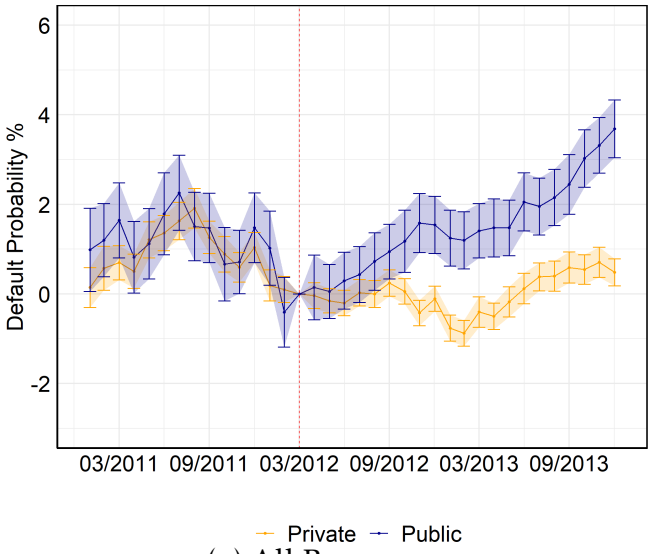
(b) Non-Exclusive Relationships



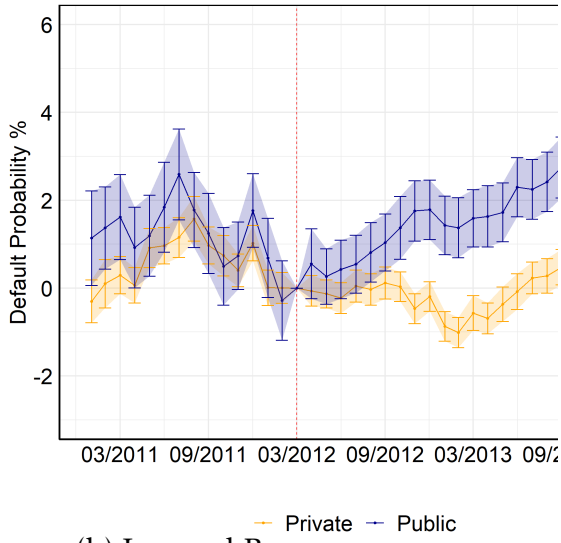
(c) Non-Exclusive Relationships, Private Debt

Figure 6: Delinquency Likelihood For Public and Private Banks

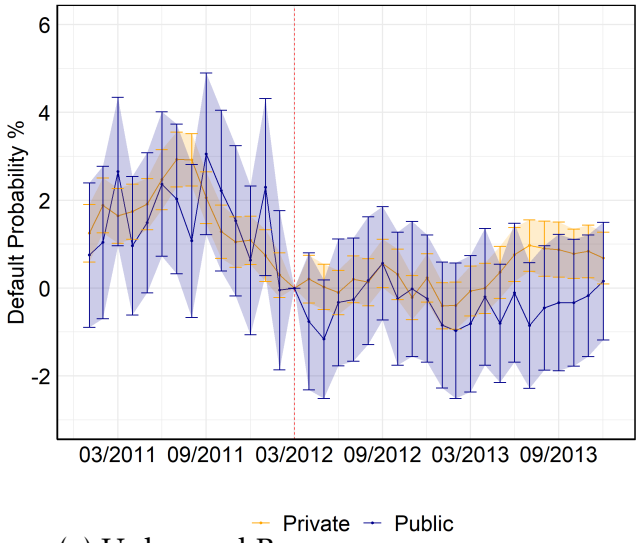
This figure shows the estimates of δ_τ from the estimation of Equation 3 at the firm-bank level for public and private banks separately for three different samples of borrowers. **Panel A:** all borrowers. **Panel B:** levered borrowers (at the moment of origination). **Panel C:** unlevered borrowers (at the moment of origination). More specifically, we run: $I^D_{tmbfs} = \alpha_{ms} + \alpha_b + \alpha_{f(size)} + \sum_{\tau \neq -1} \gamma_\tau + \varepsilon_{tmbfs}$, where I^D_{tmbf} is an indicator equal to one if a loan originated by firm f located in municipality m in month t from bank b becomes delinquent within one year after origination. Standard errors are clustered at the municipality level. Shaded areas are the 95 percent confidence intervals. Sources: Credit Information System (SCR), and authors' calculations.



(a) All Borrowers



(b) Levered Borrowers



(c) Unlevered Borrowers

Table 2: Firm Level Employment and Payroll Growth

This table shows the estimates of the β's on Equation (7) with two different dependent variables: log of employment growth (Columns 1 to 3), and log of total payroll growth (Columns 4 and 6). Each panel consists of a different sample of firms. $Public_m$ is an indicator that is one if a firm borrows from a public bank in the sample. **Panel A:** firms with exclusive relationships with public or private banks throughout the sample. **Panel B:** firms with relationships with public and private or only private banks throughout the sample. Firm size definitions are as follows. Micro firms: firms in the service/commerce sectors with fewer than 10 employees, or in the industry sectors with fewer than 20 employees. Small firms: firms in the service/commerce sectors with more than 10 and fewer than 50 employees, or in the industry sectors with more than 20 and fewer than 100 employees. Standard errors are clustered at the municipality level. All of the specification include time-industry-municipality-firm size fixed effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Sources: Credit Information System (SCR), Annual Review of Social Information (RAIS), and authors' calculations.

Panel A. Exclusive Public Bank Borrowers

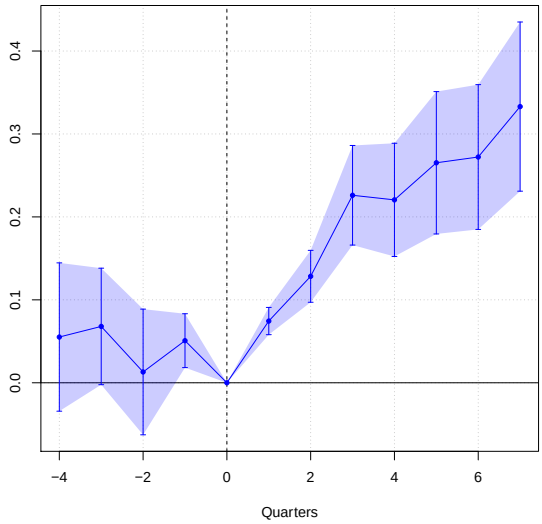
	Log Employment Growth			Log Total Payroll Growth		
	All	Micro	Small	All	Micro	Small
$Public \times I_{2012}$	0.0023	-0.0018	0.0060	0.0032	0.0001	0.0056
	(0.0064)	(0.0077)	(0.0108)	(0.0071)	(0.0090)	(0.0125)
$Public \times I_{2013}$	-0.0011	−0.0009	−0.0051	−0.0019	−0.0026	0.0006
	(0.0069)	(0.0079)	(0.0135)	(0.0074)	(0.0084)	(0.0138)
Observations	276,442	210,268	56,523	278,676	210,268	56,529

Panel B. Non-Exclusive Public Bank Borrowers

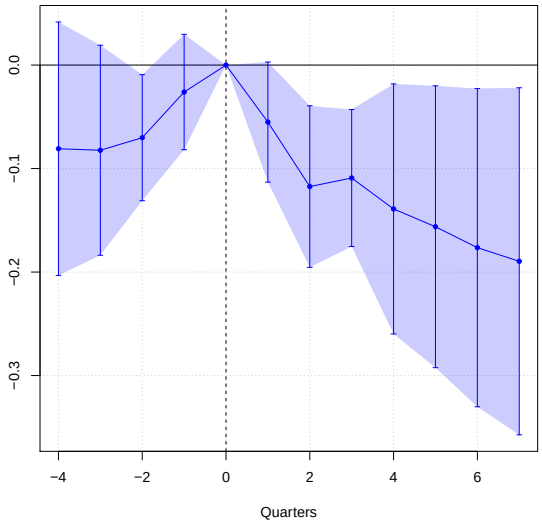
	Log Employment Growth			Log Total Payroll Growth		
	All	Micro	Small	All	Micro	Small
$Public \times I_{2012}$	−0.0009	−0.0046	-0.0030	−0.0013	−0.0058	-0.0013
	(0.0032)	(0.0035)	(0.0046)	(0.0034)	(0.0040)	(0.0052)
$Public \times I_{2013}$	0.0116***	0.0080**	0.0150***	0.0125***	0.0085**	0.0171***
	(0.0029)	(0.0032)	(0.0046)	(0.0033)	(0.0037)	(0.0051)
Observations	754,346	564,225	159,366	761,657	564,225	159,404

Figure 7: Branch Presence and Differential Credit Growth

This figure shows the estimates of the β's on Equation (5) with $Public_m$ as an indicator function that is one if municipality m has a branch from a public bank. These coefficients capture the differences in within-municipality credit evolution for municipalities with and without branches from public banks. The municipalities in this sample are those that had no bank entry after January of 2011 and that are local monopolies of either a public or private bank. Panel (a) shows the evolution of the log of the total amount outstanding at branches in a given municipality. Panel (b) shows the evolution of the log of the total amount outstanding for working capital loans for firms in a given municipality. Standard errors are clustered at the state level. Shaded areas are the 95 confidence intervals. Sources: Credit Information System (SCR), Monthly Bank Statistics by Municipality (ESTBAN), and authors' calculations.



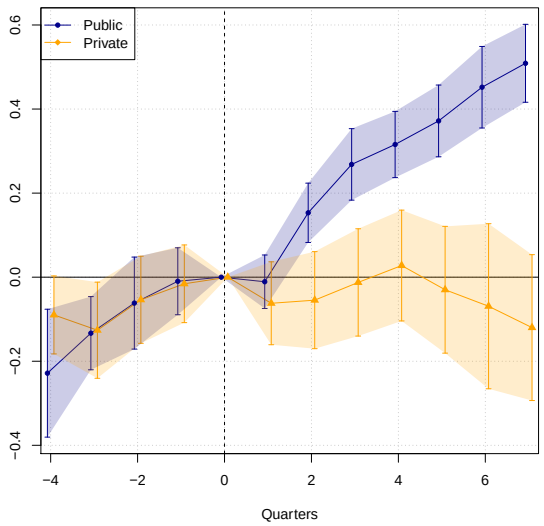
(a) Outstanding (Branches' Location)



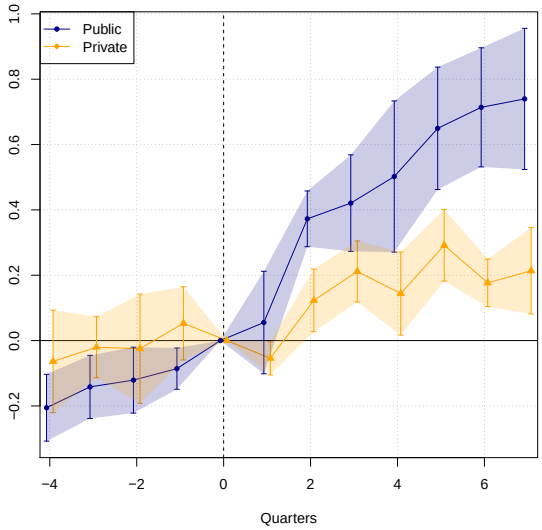
(b) Outstanding (Firms' Location)

Figure 8: Branch Presence: Working Capital Outstanding

This figure shows the estimates of the δ's on Equation (6), that is, the credit evolution for municipalities with and without branches from public banks (relative to baseline). The municipalities in this sample are those that had no previously absent bank entry after January of 2011 and that are local monopolies of either a public or private bank. Panel (a) shows the evolution of the log of the total amount outstanding of working capital for firms in municipalities with *public* branches only by public and private banks. Panel (b) shows the evolution of the log of the total amount outstanding of working capital loans for firms in municipalities with *private* branches only by public and private banks. Standard errors are clustered at the state level. Shaded areas are the 95 confidence intervals. Sources: Credit Information System (SCR), and authors' calculations.



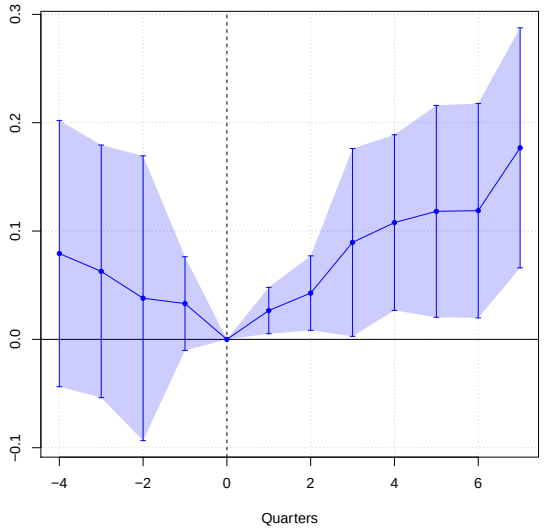
Public Branches Only



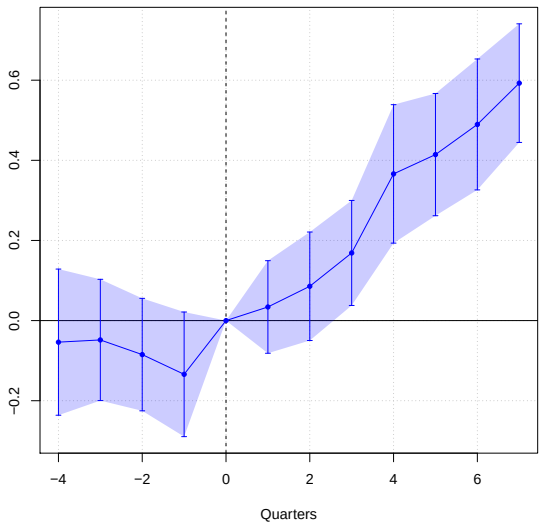
Private Branches Only

Figure 9: Pre intervention Share of Outstanding Working Capital Loans and Differential Credit Growth

This figure is equivalent to Figure 7, except for the municipality measure of exposure to the intervention. While in Figure 7 $Public_m$ is an indicator function that is one if municipality m has a branch from a public bank, here it is the share of the total amount outstanding of working capital loans that is from public banks for firms in municipality m. Standard errors are clustered at the state level. Shaded areas are the 95 confidence intervals. Sources: Credit Information System (SCR), Monthly Bank Statistics by Municipality (ESTBAN), and authors' calculations.



(a) Outstanding (Branches' Location)



(b) Outstanding (Firms' Location)

Table 3: Credit Growth at the Regional Level: Branch vs Borrower Location

This table shows the estimates of the β's on Equation (7). For Panel A, $Public_m$ is an indicator function that is one if municipality m has a branch from a public bank For Panel B, it is the share of the total amount outstanding of working capital loans that is from public banks for firms in municipality m in December of 2011. The municipalities in this sample are those that had no bank entry after January of 2011 and that are local monopolies of either a public or private bank. We run Equation (7) with three different dependent variables. Column (1): log of the amount outstanding of working capital loans for firms in a given municipality. Column (2): log of the amount outstanding of loans for firms and households in branches in a given municipality. Column (3): log of the amount outstanding of all loans for firms in a given municipality. Standard errors are clustered at the state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Sources: Credit Information System (SCR) for columns (1) and (3), Monthly Bank Statistics by Municipality (ESTBAN) for column (2), and authors' calculations.

	Panel A. Branch Presence		
	Firms	Branches	Firms
	Working Capital	All Loans	All Business Loans
Public Branch × 2012	-0.0594**	0.1723***	-0.0179
	(0.0280)	(0.0413)	(0.0520)
Public Branch × 2013	-0.1952*	0.2634***	-0.0925
	(0.1048)	(0.0670)	(0.0930)
Mun FE	Yes	Yes	Yes
Year-State FE	Yes	Yes	Yes
Observations	2,682	2,682	2,682
R^2	0.88319	0.98093	0.92491
	Panel B. Share of Working Capital Outstanding		
	Firms	Branches	Firms
	Working Capital	All Loans	All Business Loans
Public Share (2011) × 2012	0.3806**	0.0358	0.2415*
	(0.1593)	(0.0468)	(0.1334)
Public Share (2011) × 2013	0.7951***	0.1094*	0.6332***
	(0.1473)	(0.0544)	(0.1353)
Mun FE	Yes	Yes	Yes
Year-State FE	Yes	Yes	Yes
Observations	2,682	2,682	2,682
R^2	0.88776	0.97943	0.92760

Table 4: Real Effects at the Regional Level: Branch vs Borrower Location

This figure shows the estimates of the β's on Equation (7). The municipalities in the sample of Columns 1-3 are those that had no previously absent bank entry after January of 2011 and that are local monopolies of either a public or private bank. The municipalities in the sample of Columns 4-6 are those that had no previously absent bank entry after January of 2011. For Panel A, $Public_m$ is an indicator function that is one if municipality m has a branch from a public bank For Panel B, it is the share of the total amount outstanding of working capital loans that is from public banks for firms in municipality m in December of 2011. We run Equation (7) with three different dependent variables: log of GDP (Columns 1 and 4), log of employment (Columns 2 and 5), and log of total payroll (Columns 3 and 6). Standard errors are clustered at the state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Sources: Brazilian Institute of Geography and Statistics (IBGE), Annual Review of Social Information (RAIS), and authors' calculations.

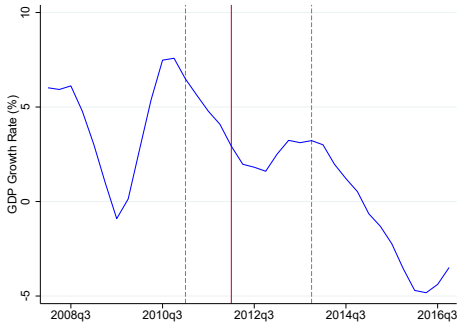
	Panel A. Branch Presence					
	Local Monopolies			**No Entry**		
	GDP	Emp.	Payroll	GDP	Emp.	Payroll
Public Branch × 2012	0.0038	-0.0039	-0.0079	0.0118	0.0050	-0.0010
	(0.0062)	(0.0138)	(0.0119)	(0.0075)	(0.0144)	(0.0132)
Public Branch × 2013	0.0003	0.0040	0.0007	0.0087	-0.0003	-0.0060
	(0.0099)	(0.0192)	(0.0175)	(0.0102)	(0.0096)	(0.0100)
Mun FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-State FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,682	2,682	2,682	8,355	8,355	8,355
R^2	0.98076	0.95300	0.9518	0.99707	0.99579	0.99535
	Panel B. Share of Working Capital Outstanding					
	Local Monopolies			**No Entry**		
	GDP	Emp.	Payroll	GDP	Emp.	Payroll
Public Share (2011) × 2012	0.0296**	-0.0109	-0.0003	0.00007	-0.0043	0.0167
	(0.0142)	(0.0273)	(0.0267)	(0.0150)	(0.0144)	(0.0135)
Public Share (2011) × 2013	0.0465**	0.0382	0.0450	0.0324***	0.0257	0.0435**
	(0.0181)	(0.0360)	(0.0318)	(0.0093)	(0.0183)	(0.0178)
Mun FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-State FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,682	2,682	2,682	8,355	8,355	8,355
R^2	0.98084	0.95309	0.95190			

Appendix

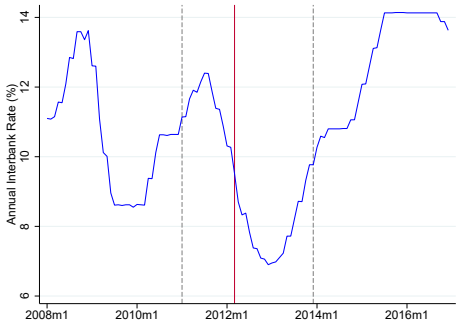
A. Additional Figures and Tables

Figure A.1: Evolution of Macroeconomic Variables

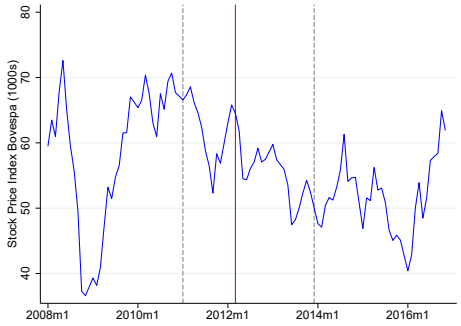
This figure shows the evolution of key macroeconomic variables during our sample. The vertical solid line denotes March 2012 or 2021Q1 for quarterly data. The dotted gray lines indicate our sample period (2011 to 2013). Panel (a) displays the Real GDP growth (seasonally adjusted). Panel (b) displays the annualized overnight interbank rates. Panel (c) displays the Bovespa Stock Price Index. Panel (d) displays the R$ per US$ exchange rate. Sources: Central Bank of Brazil/Haver Analytics, OECD/Haver Analytics, B3/Haver Analytics, and authors' calculations.



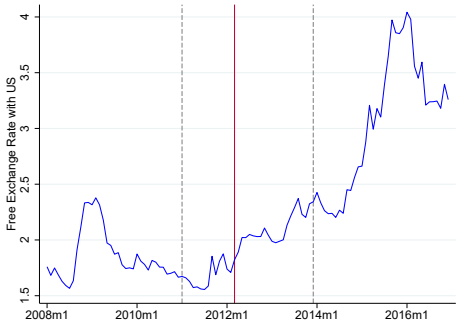
(a) Real GDP Growth



(b) Interbank Rates



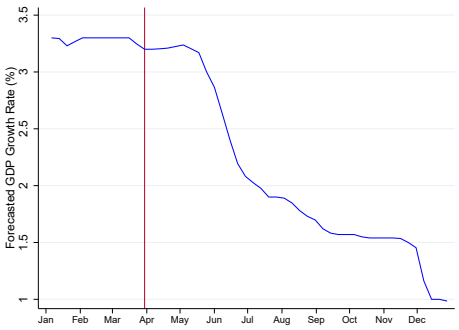
(c) Stock Index



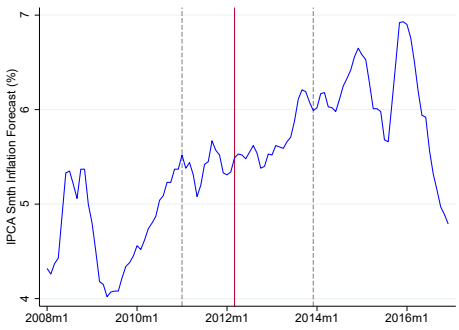
(d) Exchange Rate (R$ per US$)

Figure A.2: Forecasts of Macroeconomic Variables

This figure shows the evolution of macroeconomic forecasts during our sample period. Panel(a): GDP forecast for 2012 by month from FOCUS survey (average). Panel (b): The vertical solid line denotes March 2012. The dotted gray lines indicate our sample period (2011 to 2013). The plotted variable is 12 months ahead expected IPCA form the FOCUS survey (average). Sources: FOCUS Survey/Haver Analytics, and authors' calculations.



(a) 2012 Real GDP Growth



(b) One Year Ahead Inflation

Figure A.3: President's Net Approval Rating

This figure shows the evolution of net approval rating of Dilma Rouseff (President) from the time the took office until her impeachment. Net approval rating is defined as the percent of positive ratings minus the percent of negative ratings. The vertical solid line denotes March 2012. The dotted gray lines indicate our sample period (2011 to 2013). Sources: Reyes-Housholder (2019), and authors' calculations

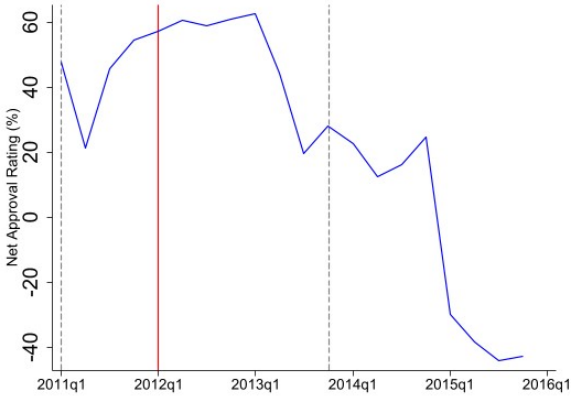


Figure A.4: Outstanding Credit by Public Banks relative to Linear Trend

This figure shows the total amount of outstanding credit and other assets by *Banco do Brasil* (BB) and *Caixa Economica Federal* (CEF) by quarter relative to the pre intervention linear trend. That is, for each t, we plot $x_t = x - \left[\frac{t}{4} \cdot (x_0 - x_{-4}) + x_0\right]$ where x_t is the total amount outstanding includes all outstanding credit to firms and households in quarter t. Quarter $t = 0$ is the start of the intervention (2012Q1) and $t = -4$ is the start of our sample. The vertical solid line indicates the start of the intervention and the vertical dashed line the end of our sample. Sources: IF.data, and authors' calculations.

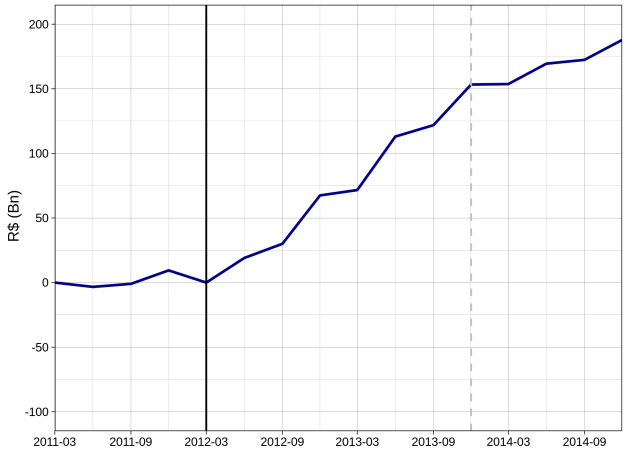
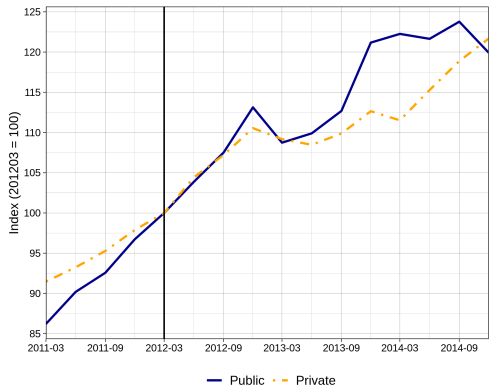
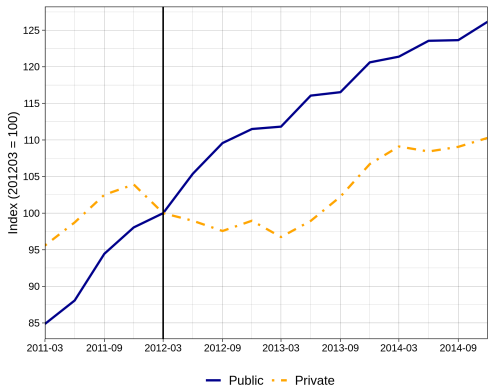


Figure A.5: Banks' Equity and Deposit Growth: Public and Large Private banks

This figure shows the ratio in deposits and equity by type of bank and quarter relative to baseline (2012Q1). *Public* (government-owned) banks are *Banco do Brasil* (BB) and *Caixa Economica Federal* (CEF). Private banks are: Bradesco, HSBC, Itau Unibanco, and Santander. Panel (a) shows the evolution of equity and Panel (b) of deposits. The vertical line indicates the start of the intervention. Sources: IF.data, and authors' calculations.



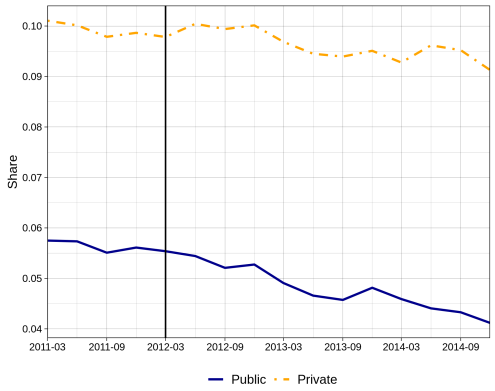
(a) Equity



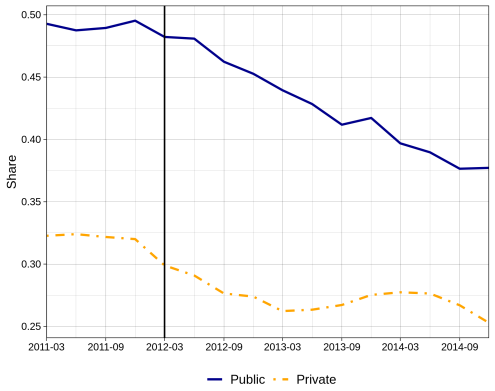
(b) Deposits

Figure A.6: Banks' Liability Decomposition: Public and Large Private banks

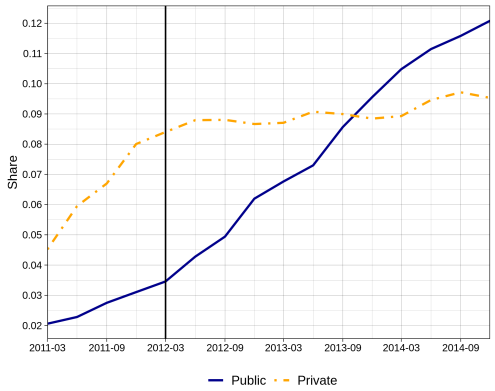
This figure decomposes the liability by bank type and quarter. *Public* (government-owned) banks are *Banco do Brasil* (BB) and *Caixa Economica Federal* (CEF). Private banks are: Bradesco, HSBC, Itau Unibanco, and Santander. Each variable is shown as a share of total liabilities. The variables are: equity (panel a), deposits (panel b), real estate, mortgage and similar notes and debentures (panel c), onlending (mostly from government funds, panel d), repurchase agreements (repos, panel e), and other liabilities (panel f). For each bank type, we compute the shares as if each type of bank is an institution, that is, the within-bank type sum of a given liability over the within-bank type sum of total liabilities. The vertical line indicates the start of the intervention. Sources: IF.data, and authors' calculations.



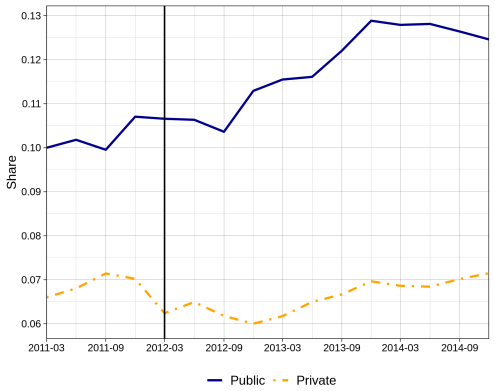
(a) Equity



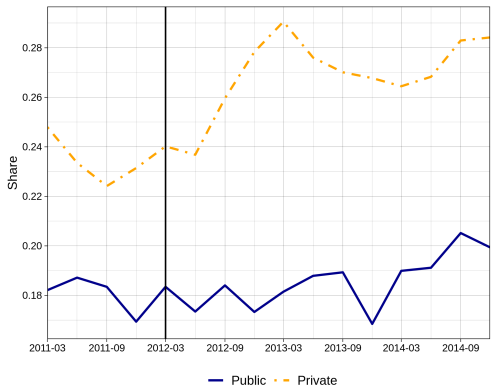
(b) Deposits



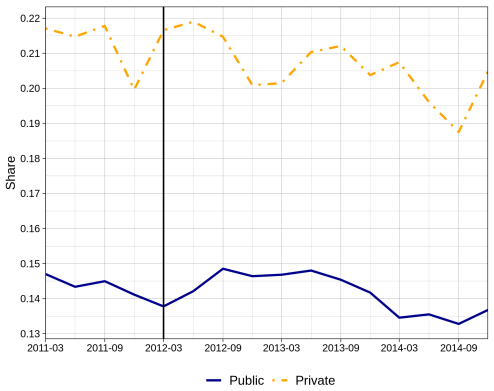
(c) Securities



(d) Onlending



(e) Repos



(f) Other

Figure A.7: Banks' Return on Equity (ROA): Public and Large Private banks

This figure shows the Return over Assets (ROA) by bank type and quarter. *Public* (government-owned) banks are *Banco do Brasil* (BB) and *Caixa Economica Federal* (CEF). Private banks are: Bradesco, HSBC, Itau Unibanco, and Santander. The returns are computed as the last four quarters net income. For each bank type, we compute the ROA as if each type of bank is an institution, that is, the within-bank type sum of net income over the within-bank type sum of assets. The vertical line indicates the start of the intervention. Sources: IF.data, and authors' calculations.

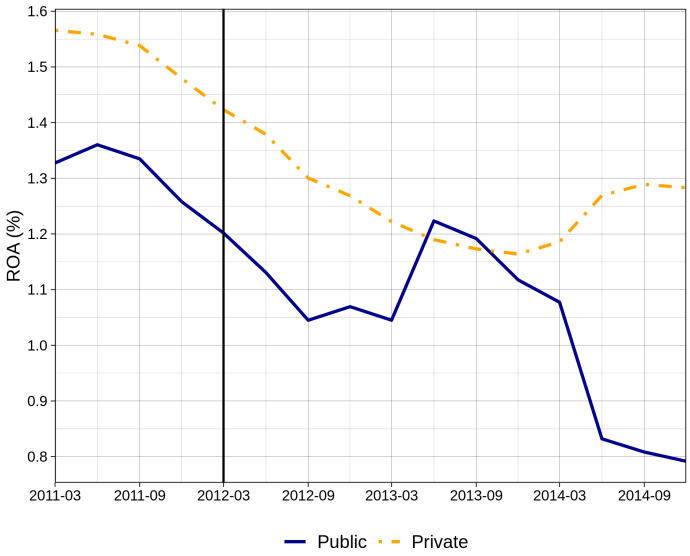
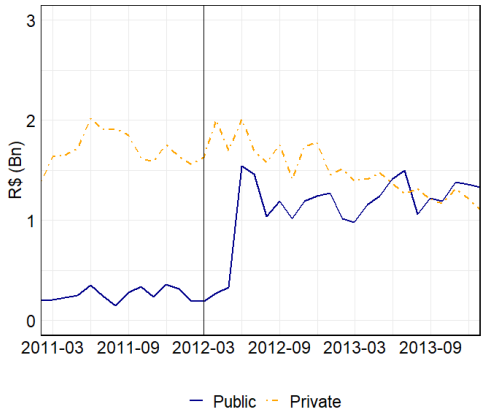
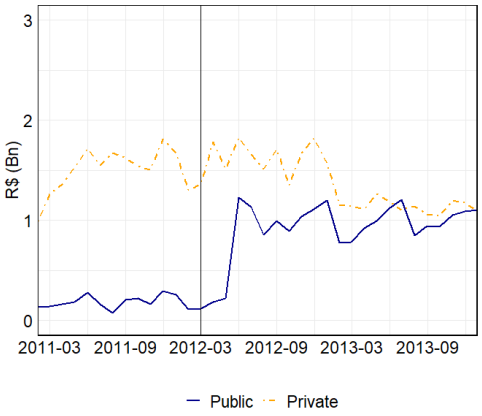


Figure A.8: Working Capital Origination by Firm Size

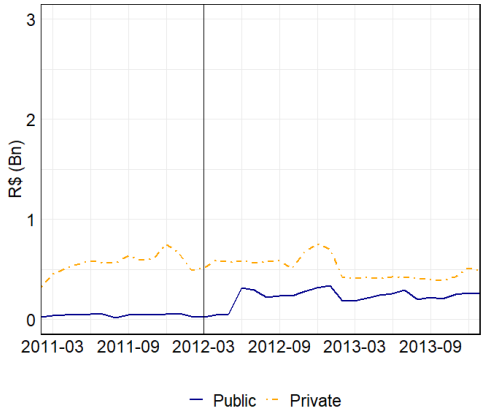
This figure shows the monthly origination of uncollateralized working capital loans to firms by type of bank and firm size. *Public* (government-owned) banks are *Banco do Brasil* (BB) and *Caixa Economica Federal* (CEF). Private banks are all other banks that are not controlled by the government. Firm-size is defined as follows. Micro firms: firms in the service/commerce sectors with fewer than 10 employees, or in the industry sectors with fewer than 20 employees. Small firms: firms in the service/commerce sectors with more than 10 and fewer than 50 employees, or in the industry sectors with more than 20 and fewer than 100 employees. Medium firms: firms in the service/commerce sectors with more than 50 and fewer than 100 employees, or in the industry sectors with more than 100 and fewer than 500 employees. The vertical line indicates the start of the intervention (March, 2012). Sources: Credit Information System (SCR), and authors' calculations.



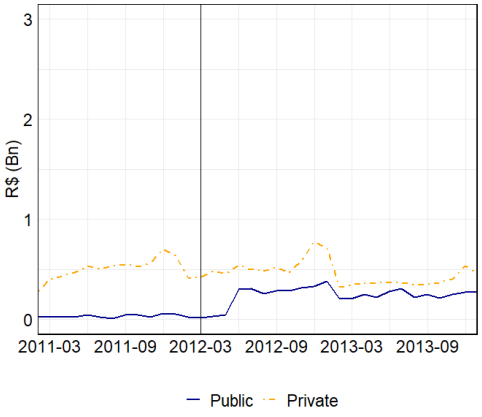
(a) Micro Firms



(c) Small Firms



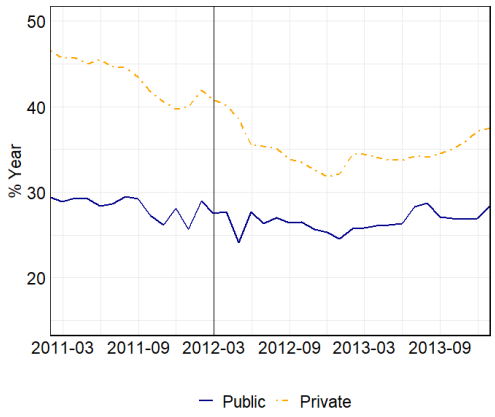
(b) Medium Firms



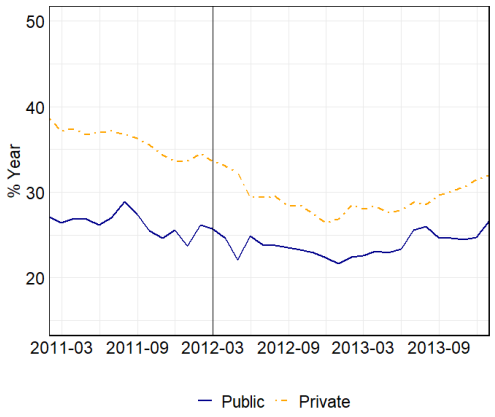
(d) Large Firms

Figure A.9: Working Capital APRs by Firm Size

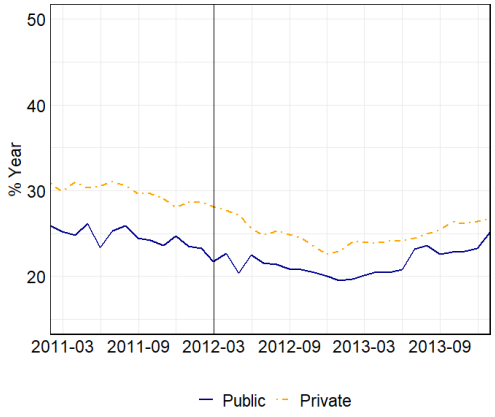
This figure shows the APR of newly originated uncollateralized working capital loans to firms by type of bank and firm size. *Public* (government-owned) banks are *Banco do Brasil* (BB) and *Caixa Economica Federal* (CEF). Private banks are all other banks that are not controlled by the government. Firm-size is defined as follows. Micro firms: firms in the service/commerce sectors with fewer than 10 employees, or in the industry sectors with fewer than 20 employees. Small firms: firms in the service/commerce sectors with more than 10 and fewer than 50 employees, or in the industry sectors with more than 20 and fewer than 100 employees. Medium firms: firms in the service/commerce sectors with more than 50 and fewer than 100 employees, or in the industry sectors with more than 100 and fewer than 500 employees. The vertical line indicates the start of the intervention (March 2012). Sources: Credit Information System (SCR), and authors' calculations.



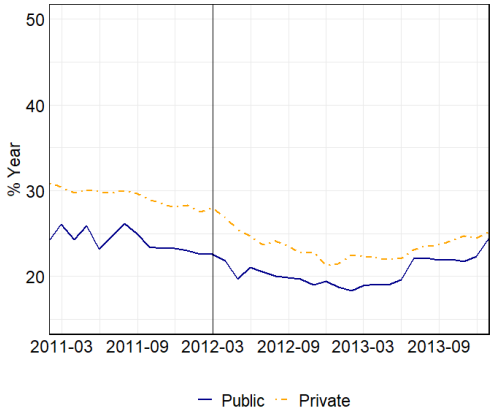
(a) Micro Firms



(b) Small Firms



(c) Medium Firms



(d) Large Firms

Figure A.10: Borrower Delinquency: Public and Private Banks

This figures shows the estimates of γ_τ from: $I^D_{tmbfs} = \alpha_{tms} + \alpha_b + \alpha_{f(size)} + \sum_{\tau \neq -1} \gamma_\tau \cdot Public_b + \varepsilon_{tmbfs}$, where I^D_{tmbfs} is an indicator equal to one if a loan originated in month t in municipality m from bank b by firm f in industry s becomes delinquent within one year after origination, α_{tms} are time-municipality-industry fixed effects, α_b are bank fixed effects, $\alpha_{f(size)}$ are firm-size fixed effects, γ_τ are time dummies, and $Public_b$ is a indicator that is one if b is a public bank. Shaded areas are the 95 confidence intervals. Standard errors are clustered at the bank-municipality level. Sources: Credit Information System (SCR), and authors' calculations.

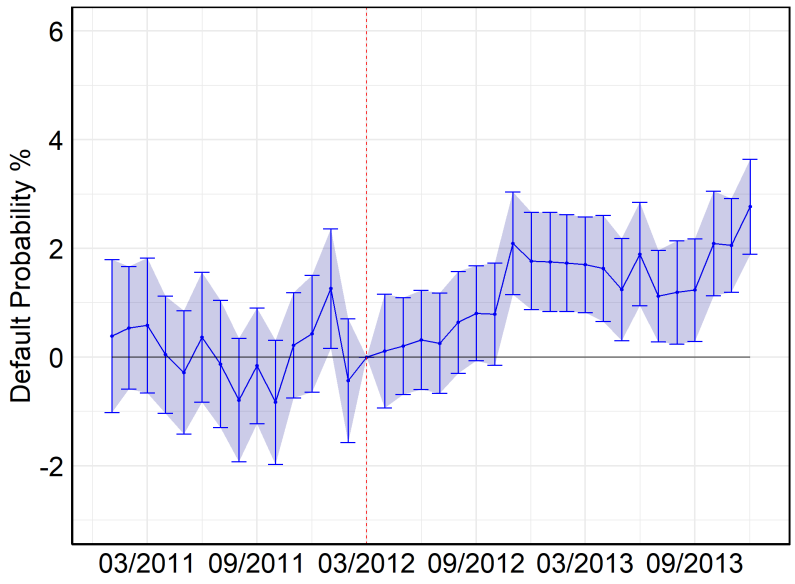


Figure A.11: Default Risk Differences: Leverage Heterogeneity by Bank Type

This figure shows the estimates of β_l in: $I^D_{tmbfs} = \alpha_{mts} + \alpha_b + \alpha_{t,f(size)} + \sum_l \beta_l \times Ind^l_f + \varepsilon_{tmbfs}$, where I^D_{tmbfs} is an indicator equal to one if a loan originated in month t in municipality m from bank b by firm f in industry s becomes delinquent within one year after origination, α_{tms} are time-municipality-industry fixed effects, α_b are bank fixed effects, $\alpha_{f(size)}$ are firm-size fixed effects, γ_τ are time dummies, and Ind^l_f are indicator variables equal to one if firm f belongs to the l-th leverage quintile. We estimate this regression at the firm-bank level for public and private banks separately. Leverage is calculated as the debt-to-payroll ratio. The first quintile is our reference category (omitted). Shaded areas are the 95 confidence intervals. Standard errors are clustered at the bank-municipality level. Sources: Credit Information System (SCR), Annual Review of Social Information (RAIS), and authors' calculations.

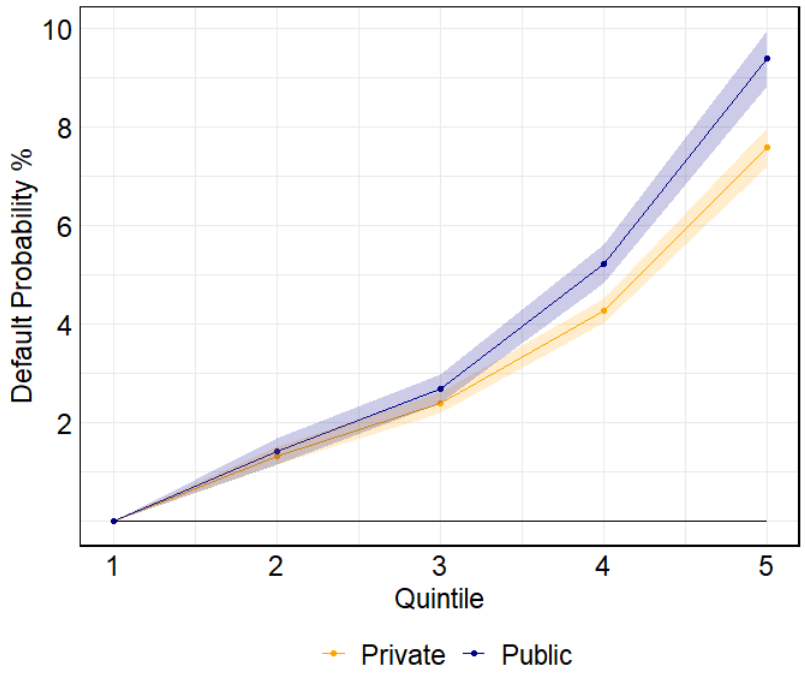


Figure A.12: Share of Originations to Unlevered Firms

This figure shows the quarterly share of originations for levered vs unlevered firms (at the time of the origination) of working capital loans to firms by type of bank. *Public* (government-owned) banks are *Banco do Brasil* (BB) and *Caixa Economica Federal* (CEF). Private banks are all other banks that are not controlled by the government. The vertical line indicates the start of the intervention. Sources: Credit Information System (SCR), and authors' calculations.

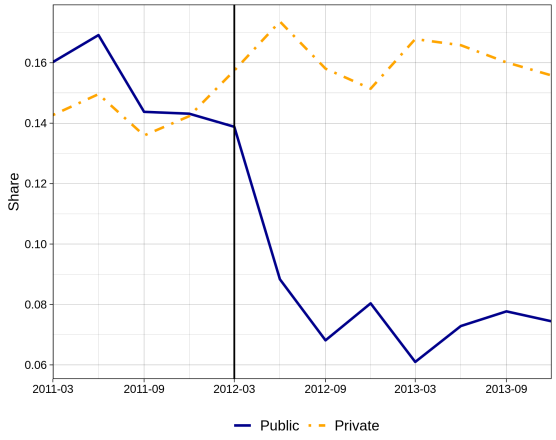


Table A.1: Credit Growth at the Regional Level: Branch vs Borrower Location (No Entry Sample)

This table shows the estimates of the β's on Equation (7). This table is equivalent to Table 3 but for a different set of municipalities. The municipalities in this sample are those that had no previously absent bank entry after January of 2011. **Panel A**: $Public_m$ is an indicator function that is one if municipality m has a branch from a public bank. **Panel B**: $Public_m$ it is the share of the total amount outstanding of working capital loans that is from public banks for firms in municipality m in December of 2011. We run Equation (7) with four different dependent variables. Column (1): log of the amount outstanding of working capital loans for firms in a given municipality. Column (2): log of the amount outstanding of loans for firms and households in branches in a given municipality. Column (3): log of the amount outstanding of all loans for firms in a given municipality. Standard errors are clustered at the state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Sources: Credit Information System (SCR), Monthly Bank Statistics by Municipality (ESTBAN), and authors' calculations.

	Panel A. Branch Presence		
	Firms	Branches	Firms
	Working Capital	All Loans	All Business Loans
Public Branch × 2012	-0.0670**	0.1423**	-0.0186
	(0.0267)	(0.0551)	(0.0320)
Public Branch × 2013	-0.1460***	0.2597***	-0.0705
	(0.0423)	(0.0720)	(0.0471)
Mun FE	Yes	Yes	Yes
Year-State FE	Yes	Yes	Yes
Observations	8,355	8,355	8,355
R^2	0.97528	0.99600	0.98172

	Panel A. Share of Working Capital Outstanding		
	Firms	Branches	Firms
	Working Capital	All Loans	All Business Loans
Public Share (2011) × 2012	0.4305***	0.0170	0.2326***
	(0.0865)	(0.0110)	(0.0561)
Public Share (2011) × 2013	0.7704***	0.0408**	0.5479***
	(0.0853)	(0.0148)	(0.0633)
Mun FE	Yes	Yes	Yes
Year-State FE	Yes	Yes	Yes
Observations	8,355	8,355	8,355
R^2	0.97665	0.99565	0.98238

Table A.2: Credit Growth at the Regional Level: Branch vs Borrower Location (No Entry Sample), Weighted By Population

This table shows the estimates of the β's on Equation (7). This table is equivalent to Table A.1 but using baseline population as weights in the estimation. **Panel A**: $Public_m$ is an indicator function that is one if municipality m has a branch from a public bank. **Panel B**: $Public_m$ it is the share of the total amount outstanding of working capital loans that is in public banks for firms in municipality m in December of 2011. We run Equation (7) with four different dependent variables. Column (1): log of the amount outstanding of working capital loans for firms in a given municipality. Column (2): log of the amount outstanding of loans for firms and households in branches in a given municipality. Column (3): log of the amount outstanding of all loans for firms in a given municipality. Standard errors are clustered at the state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Sources: Credit Information System (SCR), Monthly Bank Statistics by Municipality (ESTBAN), and authors' calculations.

	Panel A. Branch Presence		
	Firms	Branches	Firms
	Working Capital	All Loans	All Business Loans
Public Branch × 2012	-0.1346***	0.1522***	-0.0844***
	(0.0220)	(0.0481)	(0.0230)
Public Branch × 2013	-0.2056**	0.2488***	-0.0985
	(0.0989)	(0.0711)	(0.0906)
Mun FE	Yes	Yes	Yes
Year-State FE	Yes	Yes	Yes
Observations	8,355	8,355	8,355
R^2	0.99557	0.99942	0.99600
	Panel B. Share of Working Capital Outstanding		
	Firms	Branches	Firms
	Working Capital	All Loans	All Business Loans
Public Share (2011) × 2012	0.3822***	0.0629***	0.2396***
	(0.0489)	(0.0212)	(0.0382)
Public Share (2011) × 2013	0.7282***	0.1633***	0.5280***
	(0.0779)	(0.0304)	(0.0724)
Mun FE	Yes	Yes	Yes
Year-State FE	Yes	Yes	Yes
Observations	8,355	8,355	8,355
R^2	0.99592	0.99942	0.99620

Table A.3: Real Effects at the Regional Level: Branch vs Borrower Location Weighted by Population

This table shows the estimates of the β's on Equation (7). This table is equivalent to Columns 4-6 of Table 4, but using population weights in the estimation. The municipalities in the sample are those that had no previously absent bank entry after January of 2011. **Panel A**: $Public_m$ is an indicator function that is one if municipality m has a branch from a public bank. **Panel B**: $Public_m$ it is the share of the total amount outstanding of working capital loans that is from public banks for firms in municipality m in December of 2011. We run Equation (7) with three different dependent variables: log of GDP (Column 1), log of employment (Column 2), and log of total payroll (Column 3). Standard errors are clustered at the state level. $^*p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Sources: Brazilian Institute of Geography and Statistics (IBGE), Annual Review of Social Information (RAIS), and authors' calculations.

	Panel A. Branch Presence		
	GDP	Employment	Payroll
Public Branch × 2012	0.0135*	0.0137	-0.0039
	(0.0072)	(0.0359)	(0.0332)
Public Branch × 2013	0.0005	-0.0139	-0.0337
	(0.0081)	(0.0241)	(0.0205)
Mun FE	Yes	Yes	Yes
Year-State FE	Yes	Yes	Yes
Observations	8,355	8,355	8,355
R^2	0.99949	0.99940	0.99936
	Panel B. Share of WK Outstanding		
	GDP	Employment	Payroll
Public Share (2011) × 2012	0.0031	-0.0043	0.0456***
	(0.0136)	(0.0155)	(0.0137)
Public Share (2011) × 2013	0.0444**	0.0375*	0.0828***
	(0.0209)	(0.0184)	(0.0246)
Mun FE	Yes	Yes	Yes
Year-State FE	Yes	Yes	Yes
Observations	8,355	8,355	8,355
R^2	0.99949	0.99940	0.99936

Table A.4: Differences in Interest Rates by Firm Size - Public and Private Banks

This table shows the average differences in interest rates for firms of different size before and after the intervention. We estimate at the loan level the following specification: $i_{jtmbfs} = \alpha_{tms} + \alpha_b + \sum_{\tau\in\{2,3,4\}} \delta_\tau + \sum_{\tau\in\{2,3,4\}} \gamma_\tau \cdot Post_t + \varepsilon_{jtmbfs}$, where i_{jtmbfs} denotes the interest rate of a loan j issued in month t municipality m by bank b to firm f in industry s, α_{tms} are time-municipality-size fixed effects, α_b are bank fixed effects, $\tau \in \{2,3,4\}$ correspond to the size bins for small, medium and large firms, and $Post_t$ is an indicator if month t is after March 2012. Firm-size is defined as follows. Micro firms: firms in the service/commerce sectors with fewer than 10 employees, or in the industry sectors with fewer than 20 employees. Small firms: firms in the service/commerce sectors with more than 10 and fewer than 50 employees, or in the industry sectors with more than 20 and fewer than 100 employees. Medium firms: firms in the service/commerce sectors with more than 50 and fewer than 100 employees, or in the industry sectors with more than 100 and fewer than 500 employees. Coefficients δ_τ and γ_τ estimate the difference in interest rates paid by firms of size τ relative to micro firms in the baseline and post intervention periods. Standard errors clustered at the bank-municipality level. $^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. Sources: Credit Information System (SCR), Annual Review of Social Information (RAIS), and authors' calculations.

	Public Banks	Private Banks
Small Firms	-2.1506^{***}	-7.5576^{***}
	(0.0864)	(0.1099)
Medium Firms	-4.2603^{***}	-12.4970^{***}
	(0.1988)	(0.1695)
Large Firms	-5.7714^{***}	-15.5565^{***}
	(0.2496)	(0.2897)
Post × Small	-0.5971^{***}	1.3918^{***}
	(0.0855)	(0.1434)
Post × Medium	-0.5728^{***}	2.4843^{***}
	(0.1918)	(0.2053)
Post × Large	-0.5213^{***}	2.9530^{***}
	(0.2522)	(0.2735)
Time × Ind × Mun FE	Yes	Yes
Bank FE	Yes	Yes
R^2	0.332	0.355
Observations	845279	1402587

B. Regional Credit Allocation

In this section, we explore the regional credit allocation following the intervention. First, we show that there were no systematic differences across municipalities in terms of their credit growth based on political affiliations. Second, we show that other municipalities' characteristics also cannot explain the observed growth in credit.

Political Capture. There is empirical evidence that that politicians use lending by government banks to influence credit allocation and the real behavior of firms in Brazil (e.g., Carvalho (2014), Lazzarini et al. (2015)). To test if there is a political influence in the allocation of loans in our experiment, we run the following regression at the municipality m level:

$$\text{Credit Growth}_m = \alpha_s + \beta \cdot \textit{Same Party}_m + \varepsilon_m \tag{8}$$

where *Credit Growth* is a measure of credit growth, α_s are state fixed effects, and $Ally_m$ is an indicator variable if the mayor of municipality m is in the same party as the president. These data on local elections are publicly available and are provided by Superior Electoral Court (TSE). We focus the political capture analysis at the municipality level (and not at the state level, for instance) since mayoral elections took place in October 2012, while gubernatorial and presidential elections did not take place until 2014.

To account for municipality pre-intervention exposure to public banks, we use the following within-share growth measure:

$$\%\Delta^{within} Orig_m = \frac{1}{2} \cdot \frac{\text{Share Post} - \text{Share Pre}}{\text{Share Post} + \text{Share Pre}} \tag{9}$$

where shares are computed from originations in the pre and post intervention periods within bank types. Therefore, what this measure tells us is the change in credit *beyond* what would be expected if credit had a uniform expansion. For instance, if public banks had increased credit by the same percentage in all markets after the intervention, we would have that $\%\Delta^{within} Orig_{pub,m} = 0$ everywhere — and thus the allocation is not systematically geared to borrowers or branches in municipalities controlled by political allies. We compute a similar within-measure of growth for the outstanding volume of working capital loans for borrowers in a given municipality and the total amount of credit outstanding in branches in a given

municipality. Since these last two are stock variables, instead of using all periods before and after the intervention, we use simply the period before and the last period in our sample.

The results are shown in Table B.1. We show our results both unweighted and weighted by population. We do find a systematic larger increase in working capital origination or amount outstanding for borrowers in a given municipality (Panel A). We do find an increase in credit in branches located in municipalities where the mayor is a political ally, but the effect is economically small. For reference, the standard deviation across municipalities of the growth of credit outstanding in branches was 0.097. In Panel B, we extend Equation (8) to include an indicator (and its interaction with $Same\ Party_m$) if the previous election (in 2008) was contested. We define a contested election as one where there was either a second round and in the second round a candidate won with less than 55 percent of all votes, or if there was no second round, but the winner in the first round won with less than 50 percent of the votes.[32] The idea behind this exercise is that the political incentives to increase credit are larger in municipalities where elections are more competitive. We find a statistically significant increase in the outstanding volume of working capital loans, but this effect is also economically small. For reference, the standard deviation across municipalities of the growth in the outstanding volume of working capital loans was 0.19 (unweighted) and 0.11 (weighted). Note also that the share of municipalities where elections were contested and were from the same party as the president is approximately 2 percent.[33] Therefore, this heterogeneous allocation result is not driving any of the results in the main text.

Importantly, the period we analyze is marked by public bank entrance following the intervention (Figure B.1). We define bank entry as a previously absent bank opening a branch in a new municipality. When we repeat our analysis on a sample of municipalities with no bank entry, we do not find any systematically different allocation of credit. This suggests that partly the political capture channel was working through openings of new branches, rather than relative credit growth for existing branches. Our results are different from those in the Lazzarini et al. (2015), Carvalho (2014) and others in the government banks literature. This distinction comes from two sources. First, differently from the firms evaluated in these papers, the firms in our sample are small, and thus unlikely to have political connections.

[32] Municipalities with fewer than 200,000 residents do not have second rounds for mayoral elections.
[33] We arrive at a similar figure on the share of the population in these municipalities.

Second, the intervention we analyze takes place close to a mayoral election, but we do find significant spillovers across municipalities in terms of credit allocation.

Other Characteristics. We conduct a similar analysis with other municipality characteristics (before the intervention) to understand if there were other systematic differences in the allocation of credit. To do so, we replace the right-hand variable in Equation (8). The results are shown in Table B.3. We focus in the within-growth in the outstanding amount of working capital loans for firms in a given municipality for this result. The overall patterns are robust if we use our difference measures of local credit growth. We do not find any economically significant difference in credit allocation across our samples. For reference, the standard deviation of the HHI of private credit, credit Per capita and Industrial Share (Weighted) are, respectively, 0.42, 2.94, and 0.14. Given that the standard deviation in our measure of credit growth is 0.19 (unweighted) and 0.11 (weighted), a one standard deviation change in any of these statistically significant coefficients does not represent a significant increase in credit growth. Moreover, these effects are not robust across samples and weighting schemes. Note that this does not imply that the allocation was not heterogeneous across municipalities, but rather that it wasn't systematically heterogeneous *beyond* baseline exposure. We document the dependence of the allocation on baseline exposure in Section V.2, and use it as a source of variation to estimate the regional effects of the intervention.

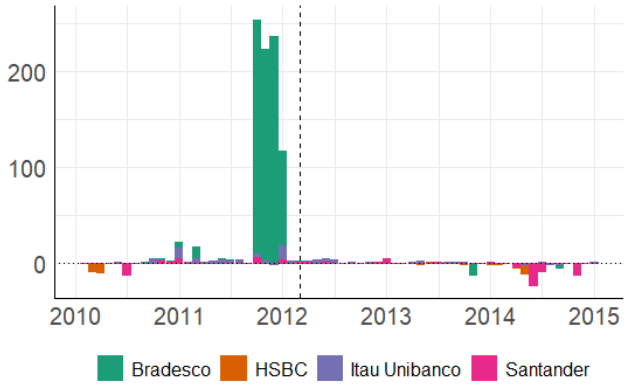
Table B.1: Political Capture and Credit Growth

This table shows the estimates of the β's from estimating regression in Equation (8). In all cases, we run this regression with state fixed effects, α_s. We run this regression with six different dependent variables. These are based on working capital originations (*WK Originations*) for firms in that municipality, working capital outstanding (*WK Outstanding*) for firms in that municipality, and total credit outstanding in branches from that municipality (*Outstanding (Branches)*). We compute these for public banks only and for both types of banks. For the *WK Originations*, we use the total amount originated in pre and post periods as described in Equation (9). For the outstanding measures, we use the baseline and end of the sample amount of credit. The $Same\ Party_m$ variable is an indicator if the mayor of municipality m is from the same party as the president. The *Contested* variable is an indicator if the 2008 election was contested (see text for definition). The weights for panels C and D are population (baseline). Standard errors are clustered by state in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Sources: Superior Electoral Court (TSE), Credit Information System (SCR), Monthly Bank Statistics by Municipality (ESTBAN) and authors' calculations.

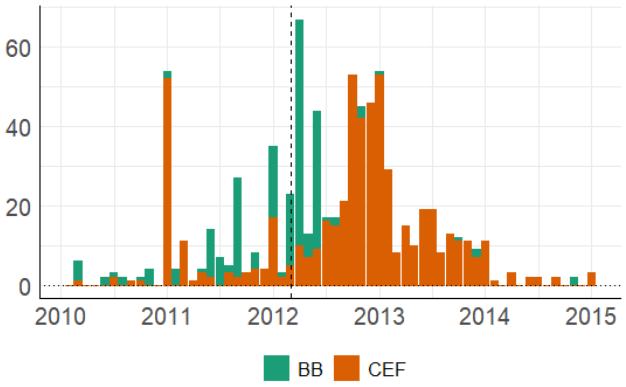
	WK Originations		WK Outstanding		Outstanding (Branches)	
	Public	All	Public	All	Public	All
	Panel A: Level, Unweighted					
Same Party	0.0055	0.0040	-0.0006	-0.0127*	0.0074**	0.0059
	(0.0129)	(0.0094)	(0.0095)	(0.0073)	(0.0031)	(0.0039)
Obs	4,618	4,618	4,618	4,618	4,618	4,618
	Panel B: Interaction, Unweighted					
Same Party × Contested	-0.0050	0.0246	0.0440*	0.0420**	0.0012	0.0026
	(0.0390)	(0.0196)	(0.0217)	(0.0195)	(0.0142)	(0.0072)
Obs	4,618	4,618	4,618	4,618	4,618	4,618
	Panel C: Level, Weighted					
Same Party	-0.0029	-0.0183**	0.0157	0.0025	0.0063	-0.0037
	(0.0082)	(0.0081)	(0.0105)	(0.0065)	(0.0060)	(0.0049)
Obs	4,618	4,618	4,618	4,618	4,618	4,618
	Panel D: Interaction, Weighted					
Same Party × Contested	0.0042	0.0255	0.0315*	0.0121	-0.0006	0.0083*
	(0.0269)	(0.0172)	(0.0183)	(0.0140)	(0.0093)	(0.0045)
Obs	4,618	4,618	4,618	4,618	4,618	4,618

Figure B.1: Branch Openings in New Municipalities

This figure shows branch openings in new municipalities by the five largest private banks and the two public banks that are the focus of our study. Entry is defined by a bank opening a branch in location where it had no previous presence. Sources: Monthly Bank Statistics by Municipality (ESTBAN), and authors' calculations.



Private Banks



Public Banks

Table B.2: Political Capture and Credit Growth: No Entry Sample

This table is equivalent to Table B.1. The only difference is that we include only municipalities that had no branch openings in our sample. Standard errors are clustered by state in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Sources: Superior Electoral Court (TSE), Credit Information System (SCR), Monthly Bank Statistics by Municipality (ESTBAN) and authors' calculations.

	WK Originations		WK Outstanding		Outstanding (Branches)	
	Public	All	Public	All	Public	All
	Panel A: Level, Unweighted					
Same Party	0.0053	0.0013	-0.0047	-0.0209**	0.0040*	0.0023
	(0.0133)	(0.0091)	(0.0083)	(0.0077)	(0.0022)	(0.0021)
Obs	3,881	3,881	3,881	3,881	3,881	3,881
	Panel B: Interaction, Unweighted					
Same Party × Contested	-0.0107	0.0213	0.0348*	0.0381*	-0.0066**	-0.0016
	(0.0351)	(0.0258)	(0.0200)	(0.0198)	(0.0026)	(0.0037)
Obs	3,881	3,881	3,881	3,881	3,881	3,881
	Panel C: Level, Weighted					
Same Party	-0.0063	-0.0192**	0.0134	0.0011	0.0069	-0.0041
	(0.0077)	(0.0070)	(0.0105)	(0.0069)	(0.0062)	(0.0038)
Obs	3,881	3,881	3,881	3,881	3,881	3,881
	Panel D: Interaction, Weighted					
Same Party × Contested	0.0015	0.0228	0.0267	0.0055	-0.0058	0.0038
	(0.0228)	(0.0189)	(0.0169)	(0.0142)	(0.0073)	(0.0056)
Obs	3,881	3,881	3,881	3,881	3,881	3,881

Table B.3: Baseline Regional Characteristics and Credit Growth

This table shows the estimates of the β's from estimating regression in Equation (8) but with different dependent variables. We run this regression with state fixed effects, α_s. The dependent variable is the within-share growth of working capital outstanding (*WK Outstanding*) for firms in that municipality as described in Equation (9) using the baseline and end of the sample amount of credit. The right hand side variables in this case are municipality characteristics measured at the baseline. These are: GDP per Capita (R$ 1,000), the HHI of Private Credit (measured from ESTBAN), the share of output from the agricultural and industrial sectors and Total Credit Per Capita (R$ 1,000, measured from ESTBAN). The weights for the last two columns are population (baseline). The independent Standard errors are clustered by state in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Sources: Superior Electoral Court (TSE), Credit Information System (SCR), Monthly Bank Statistics by Municipality (ESTBAN) and authors' calculations.

	Unweighted		Weighted	
	All Municipalities	No Entry	All Municipalities	No Entry
GDP per Capita (R$ 1,000)	-4.9×10^{-5}	-9.49×10^{-5}	-9.63×10^{-5}	1.16×10^{-6}
	(0.0001)	(0.0001)	(0.0002)	(0.0007)
HHI Private Credit	0.0205***	0.0272***	-0.0294	0.0613
	(0.0055)	(0.0097)	(0.0350)	(0.0371)
Agricultural Share	-0.0401	-0.0311	-0.0338	-0.0191
	(0.0277)	(0.0290)	(0.0463)	(0.0828)
Industrial Share	0.0140	0.0103	0.0688*	-0.0025
	(0.0225)	(0.0220)	(0.0364)	(0.0335)
Credit per Capita (R$ 1,000)	-0.0017*	-0.0016*	-0.0005	-0.0010
	(0.0010)	(0.0010)	(0.0026)	(0.0020)